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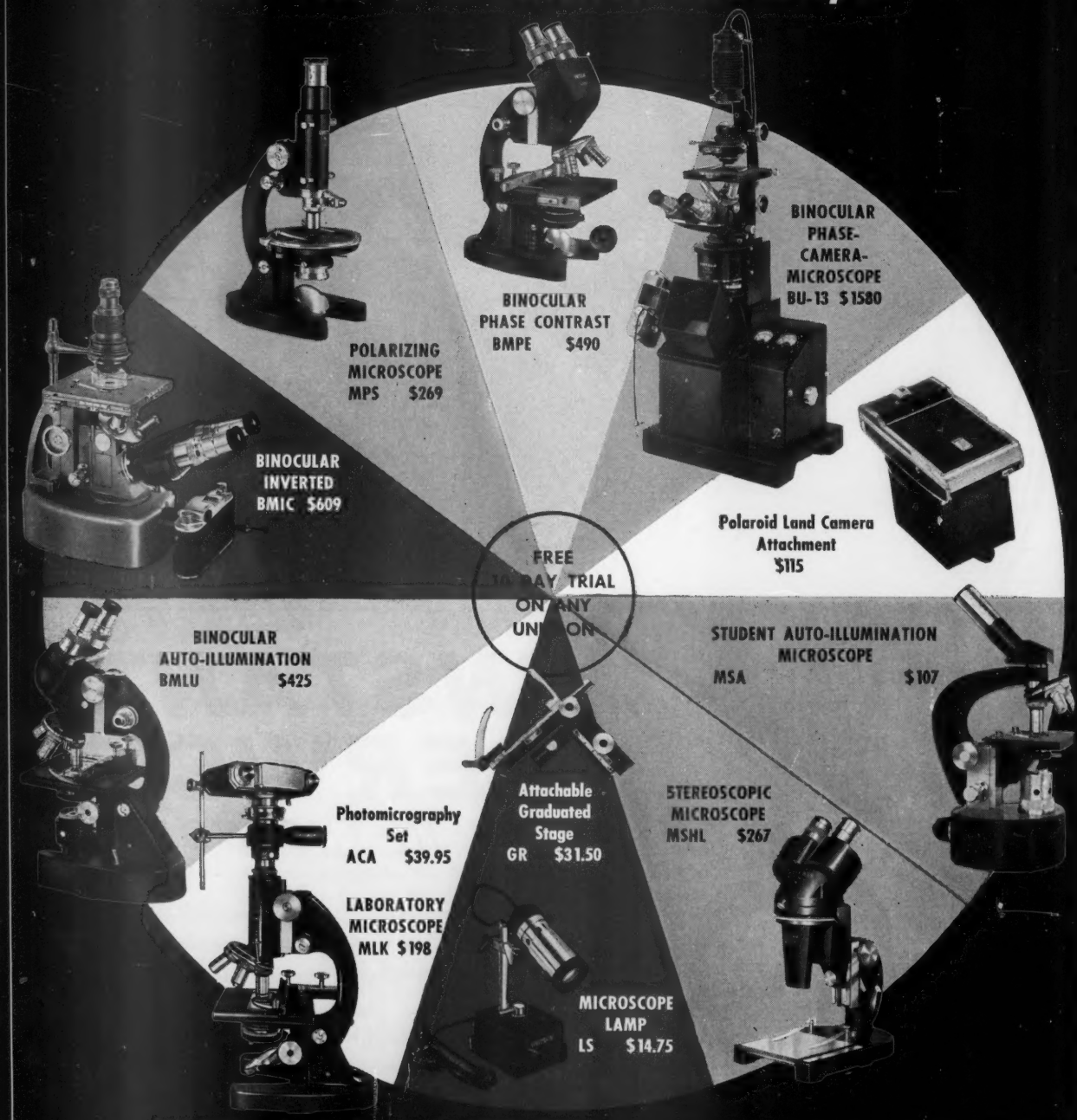

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14 April 1961, Volume 133, Number 3459

SCIENCE

Editorial

Diversity in Research Support 1103

Articles

Geologic Time Scale: *J. L. Kulp* 1105

Isotopic age determinations on rocks of known stratigraphic age define an absolute time scale for earth history.

The Moon as a Collector of Biological Material: *E. Anders* 1115

Terrestrial microorganisms may be found on the moon, but extra-solar-system biota would be undetectable.

Science in the News

Disarmament Review: It Will Clarify Some Touchy General Points, But Working Out the Details Is Difficult 1117

Book Reviews

C. Bibby's *T. H. Huxley, Scientist, Humanist and Educator,* reviewed by *B. Glass;* other reviews 1122

Reports

Isolation of *Blastomyces dermatitidis* from Soil: *J. F. Denton* et al. 1126

Ability of Some Black Sea Organisms To Accumulate Fission Products: *G. G. Polikarpov* 1127

Slowing of Heart Rate after Septal Self-Stimulation in Rats: *R. B. Malmo* 1128

Fallout from Nuclear Detonations of February and April 1960: *P. K. Kuroda, H. L. Hodges, H. E. Moore* 1130

Blockade of Deoxyribonucleic Acid Synthesis by Deuterium Oxide: *P. R. Gross* and *C. V. Harding* 1131

Bulbar Control of Cortical Arousal: *M. Bonvallet* and *V. Bloch* 1133

Departments

Letters from *L. A. Romo; R. W. Erickson; J. R. Kuppers* 1136

Forthcoming Events 1140

Cover

Nickel-zinc alloy etched with hydrochloric acid and water (polarized light, × 750). [Research Laboratory, General Electric Company, Schenectady, N.Y.]

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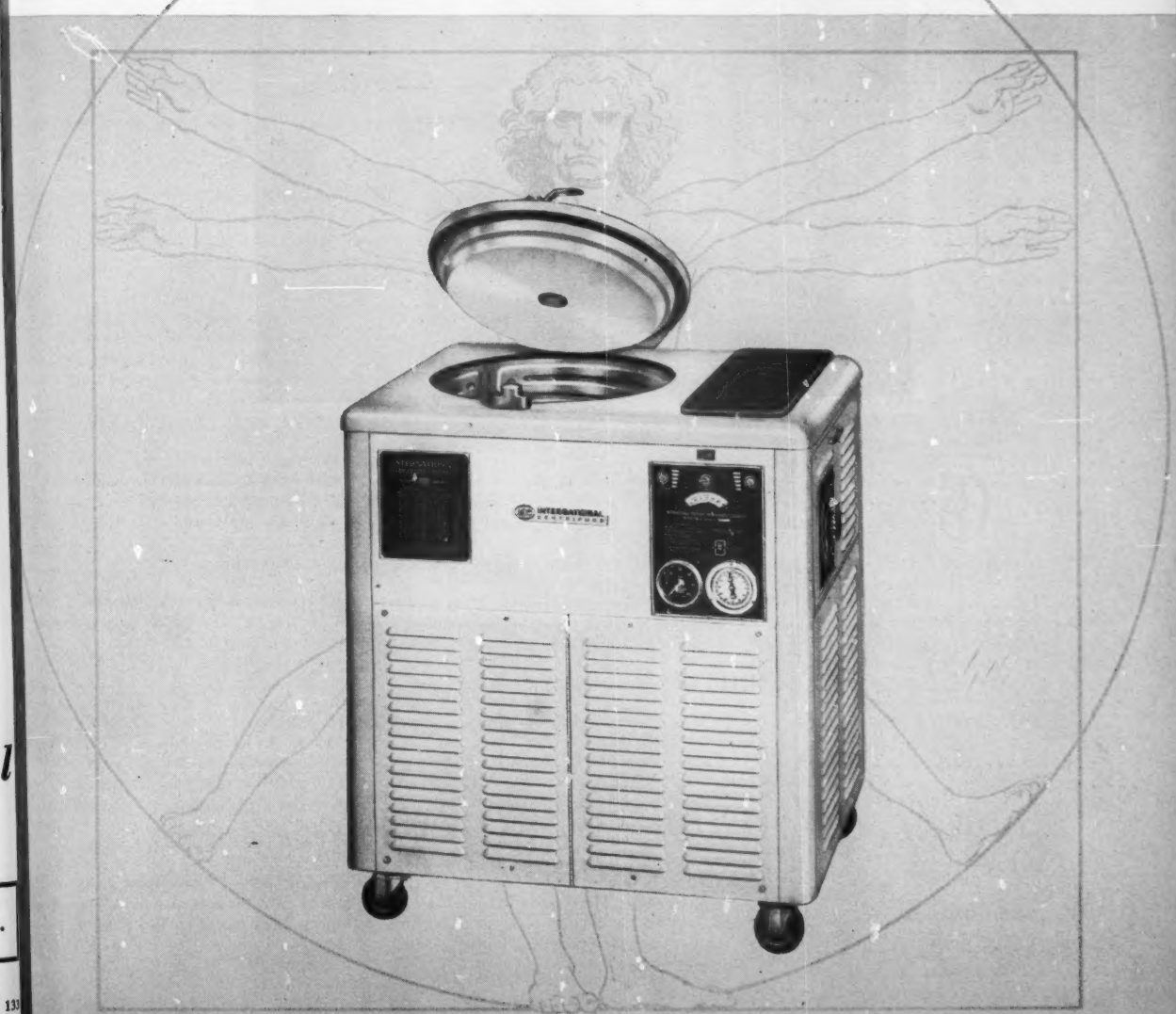

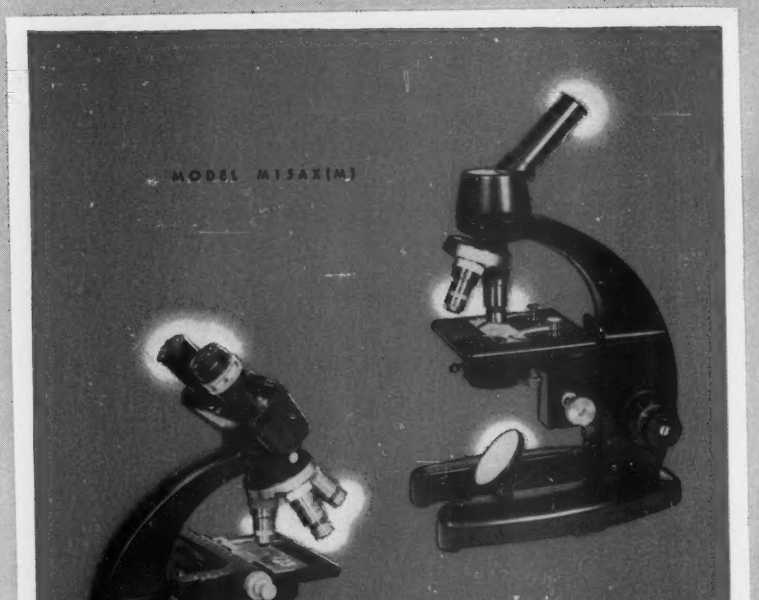

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Thermoelectricity, Electrical Resistance and Electron Scattering in Metal Alloys

How does the scattering of electrons affect the thermoelectric behavior and the conduction of electricity in metals? Physicists at Honeywell have been able to successfully predict certain features of this behavior on the basis of a simple mathematical model.

Everyone is familiar with the fact that metals are good conductors of electricity and also good conductors of heat. All of the electrical current and most of the heat is carried ordinarily by the electrons in a metal. The flow of electricity is intimately associated with the flow of heat or energy, and we can say that the heat flow "interacts" with the electrical current flow. Whatever gives rise to one of these flows will also give rise to the other. Thus if we connect a wire across a battery we get a flow of energy (heat) as well as a flow of electricity; conversely, if we place one end of a copper wire in a flame, heat will pass down the wire and at the same time an electrical current will flow momentarily so as to "readapt" the electrons to the new situation in the wire—one end of the wire is now hotter than the other. The electrical current flow is only momentary because there is no "return path" by which the electrons can flow around in a closed circuit.

If we connect an iron wire to the copper wire at the hot end, and connect the two free ends to a sensitive voltmeter, there will be a flow of heat (energy) in both wires, of course, and there will also be a flow of electrons in both wires. But now, since the electrons are free to traverse a closed loop (through the voltmeter), the electrical current flow is sustained as long as we keep the hot junction in the flame.

The experimental facts that heat flow in a pair of wires (called a *thermocouple*) can cause electricity to flow has been known for over a century and is called the Seebeck Effect; the reverse phenomenon, that electrical current flow through a junction of dissimilar metals will cause heat to flow toward or away from the junction, depending upon the *direction* of the current, has been known for nearly a century and is called the Peltier Effect. The Seebeck Effect is used in thermocouples to measure temperature, to detect radiation (which simply heats the junction), to generate electricity from heat, and for many other related applications which involve the conversion of heat into electrical current flow. The Peltier Effect has only recently come into prominence in thermoelectric refrigerators, and in these applications as well as in thermoelectric generators it has been found that semiconductors are often more suitable than metals.

Although the general connection between heat flow and electrical current flow is now well known, the detailed *mechanisms* of electrical and heat conduction in metals is only poorly understood. The extent to which a particular metal conducts electricity or heat is determined in part by the number of electrons which can contribute to the conduction process, and in part by the opposition or "resistance" which these electrons meet as they move along the wire. This resistance results from the presence of various kinds of "obstacles" or "irregularities" in the wire and can be treated as a *scattering* of the electrons from their normal forward motion along the wire. For example, a "perfect" crystal lattice consisting of only copper atoms at *fixed* positions would present no resistance to the flow of electrons; but at any temperature above absolute zero the copper atoms vibrate about their normal positions and behave as "obstacles" to the conduction electrons.

A very effective kind of obstacle or scatterer is an atom of a kind different from that of which the crystal itself is made. Thus if we place a gold atom in a copper lattice, the electrons are confronted with an "oddity" in the otherwise "pure" copper lattice and are thereby scattered: the more gold atoms, the larger the total scattering and the larger the electrical resistivity. Also, some atoms are more effective scatterers than others; thus iron atoms in copper are nearly twenty times more effective than gold atoms. Finally, the scattering ability of a given atom usually depends upon the temperature of the crystal.

Our understanding of the facts just described is at best only qualitative, and the specific *interactions* between heat flow and electrical flow which give rise to the Seebeck and Peltier thermoelectric effects are even more of a mystery! Nevertheless, we have been successful in predicting the thermoelectric behavior of *ternary* alloys on the basis of empirical information on *binary* alloys. We have thus contributed some small amount of order into a technological field (metal thermocouples) which for a century has been within the exclusive domain of alchemy.

But our motivation in this work has not been *primarily* the development of "bigger and better" resistance and thermocouple materials—rather it has been a desire to understand specifically how electrons intereact with various kinds of imperfections in a crystal. Together with physicists, chemists and metallurgists in other laboratories throughout the world, we look forward to a more thorough understanding of this very small facet in the fascinating study of matter.

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Diversity in Research Support

Philanthropy, though not an exclusively American phenomenon, has had its most exuberant manifestations in this country. The larger endowed foundations—those with assets of $50,000 or more—include such giants as the Ford, the Rockefeller, and the Carnegie Foundations and number more than 5200. Their total assets exceed $11 billion. Among the 7000 philanthropic organizations not included in these totals are those that have little or no endowment and that depend for funds upon annual public appeals. In this category are the voluntary health agencies, such as the National Foundation, the American Cancer Society, and the American Heart Association.

The voluntary health agencies typically concentrate on one or, at most, several diseases and put on annual public fund-raising campaigns. Some devote the major share of their efforts to public education and to care and rehabilitation of patients; others, in addition to these functions, support research and research training in their areas of interest; still others focus exclusively on public education and research.

These agencies have made important contributions. Although criticized by some for overdramatizing the battle against particular diseases, they can show that this has been the only effective way to marshal support from the general public. They have pioneered in educating the public in the prevention and treatment of disease; in creating greater public understanding of the importance and promise of research; in ameliorating or conquering disease; and in developing effective methods for allocating research grants and fellowships. The American Cancer Society initiated an institutional grants program—one widely adopted by other agencies and authorized (but not funded) for the National Institutes of Health by Congress. The American Heart Association pioneered in making career awards—essentially lifetime professorships—for investigators of exceptional ability, and the American Cancer Society adopted a similar program. The NIH has followed suit, except that its career professorships are limited to a 5-year term, and that it has a moral but not a legal commitment to renew appointments.

Not the least of the accomplishments of the voluntary agencies is the part they have played in persuading Congress to establish the NIH within the U.S. Public Health Service. Starting with a small appropriation in 1945, the NIH—with strong support by representatives of these agencies—has enjoyed a remarkably rapid growth in appropriations: from $52 million in 1950 to more than $400 million in 1960.

But it is this very growth of the federal effort in research that poses a threat to the voluntary health agencies. Of the $150 million spent on cancer research in 1960, $91 million came from the National Cancer Institute, $10.5 million from the American Cancer Society, and the rest from other governmental agencies and private foundations. The same holds in other categories: the government is footing well over half the research bill.

Overlapping support is not the problem, since there is complete exchange of information between all major granting agencies. The problem is that the general public, aware of the increasing federal contribution, no longer sees what part the voluntary agencies may have to play. To show the public that diversity of support is essential to flexibility and creativity in research, and that we would all be the losers if this diversity were lost, is no easy task. But it is a task that ought to be undertaken, and one that is worthy of our best efforts.—G.DuS.

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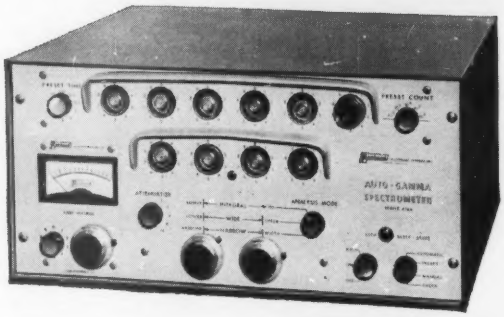

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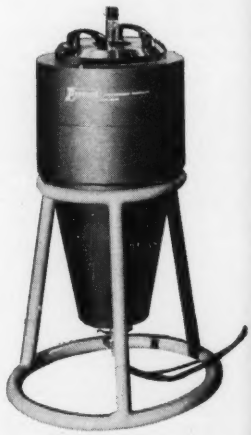

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14 April 1961, Volume 133, Number 3459 SCIENCE

Geologic Time Scale

Isotopic age determinations on rocks of known stratigraphic age define an absolute time scale for earth history.

J. Laurence Kulp

The concept that earth history has encompassed millions of years became established with the development of the geological sciences. Since the time dimension is central to geology, quantitative measurement of time is necessary if earth processes are to be fully understood. Such measurement became theoretically possible with the discovery of radioactivity, but it is only with the developments of the past decade in theory and technique that it has become possible to make reasonably accurate determinations.

The term *g ological time scale* generally refers to the absolute calendar to which the progressive development of animals and plants may be related. The interval of earth history involved is from the Cambrian period to the present. The Cambrian is the earliest period for which detailed faunas can be described. Isotopic age measurements have been widely used to determine the time of formation of igneous and metamorphic rocks in the seven-eighths of geologic history that occurred before the Cambrian, but utilization of such measurements to determine the absolute age of fossil-bearing sedimentary rocks has been slow to develop, because of problems of measurement and of suitability of materials or geologic settings.

The author is professor of geochemistry at Columbia University and director of the geochemistry laboratory, Lamont Geological Observatory, Palisades, N.Y.

In Table 1, the earlier estimates of the geological time scale from potential age measurement based on radioactive decay are compared with the new data at several key points. Holmes's 1933 and 1947 estimates (*1*, *2*) came to be used by all earth scientists. The agreement between these and the current estimates is surprising and obscures the fact that the new scale is far more accurate than the earlier ones. When Barrell (*3*) compiled his time scale it was not even recognized that thorium as well as uranium produces radiogenic lead. In the 1933 scale of Holmes there was still no correction for the presence of common lead in uranium minerals. Holmes's 1947 scale was based on complete isotopic determinations of five samples carefully analyzed by A. O. C. Nier, but at that time the effects of geochemical alteration on these isotopic ages were not adequately appreciated. Unfortunately, it was subsequently shown that two of these points carried incorrect stratigraphic assignments (Bedford, N.Y., and Middletown, Conn.) and that two others yielded incorrect ages because of geochemical alteration (Swedish kolm and Joachimsthal pitchblende). In retrospect, it seems largely fortuitous that these early estimates of the time scale were fair approximations of the actual scale.

No significant improvement in the construction of the geologic time scale could be made until the new isotopic geochronometers based on the radioactive decay of rubidium-87 and potassium-40 were discovered and until improved analytical techniques for all methods were developed. It is only within the past few years that suitable measurements based on these methods have been reported. Thus, a reconsideration of calendars of geologic events was warranted.

Recent preliminary revisions of the time scale, based in large part on data available in early 1959, have been published by Kulp (*4*) and by Holmes (*5*). A large body of critical data has become available since these reports were written, however, and the causes of geochemical alteration are now more fully understood, so that it is possible to examine the time scale in much greater detail. In 1959 Mayne *et al.* (*6*) proposed a greatly extended time scale based essentially on two new experimental points—Hercynian and Caledonian granites from Britain—but these measurements and the supporting arguments have been shown to be incorrect (*7–9*). Evernden *et al.* (*10*) have suggested a time scale back to the Jurassic, based on their extensive and excellent measurements of ash beds and flows, made largely on continental rocks from the western United States.

Folinsbee and his co-workers (*11*) have contributed some very important measurements, particularly for the Cretaceous, and suggest that the Holmes 1947 time scale must be lengthened. This is also the conclusion of Hurley *et al.* (*12*) on the basis of measurements in New England and Nova Scotia on rocks of Devonian and Carboniferous-Permian age. Faul (*13*) considered the time-scale problem and concluded that not enough information was available to merit a serious revision. Some of the anomalies which led him to this conclusion, however, were due to uncertain stratigraphic definition, the inclusion of a number of Cambrian glauconites which surely have lost argon, and the measurements of Mayne *et al.* (*6*). Many new measurements, together with clarification of some of the problems raised by Faul (*13*), make it possible to estimate the time boundaries

Table 1. Comparison of earlier and current estimates of certain points in the geological time scale.

Geologic period	Date of beginning of period ($\times 10^6$ yr)			
	Barrell, 1917 (*3*)	Holmes, 1933 (*1*)	Holmes, 1947 (*2*)	This study
Tertiary	55–65	60	58–68	63 ± 2
Jurassic	155–195	158	152–167	181 ± 5
Carboniferous	300–370	285	255–275	345 ± 6
Ordovician	480–590	440	430	500 ± 20

for most of the geologic periods, and in some cases the epochs, within relatively narrow limits. It is the purpose of this article (*14*) to evaluate the available pertinent measurements and from these data to construct the most probable geological time scale.

Methods

The isotopic chronometers that are applicable to this problem are listed in Table 2. The decay constants are known to within at least 5 percent, and in the case of uranium-238, uranium-235, and rubidium-87 the error is probably less than 3 percent. Any small change will, of course, shift all ages from one chronometer relative to another. At present, different mineral phases from the same rock, regardless of age, may give isotopic ages based on the decay schemes of uranium, rubidium, and potassium that agree within the limits of analytical error; hence, no major change in the decay constants is expected.

For each measurement there are three areas of uncertainty: (i) the analysis, (ii) the degree to which the mineral sample has remained a closed chemical system, and (iii) the stratigraphic definition of the sample. By using isotope dilution techniques, virtually all of these isotopic ratios can be determined to within 3 percent. Standard errors as low as 1 percent can be obtained. For example, five laboratories in the United States analyzed a biotite standard supplied by the department of geophysics of Massachusetts Institute of Technology. The reported values for the radiogenic argon-40 concentration were all within 1 percent of the mean. For a critical sample, replicate analyses are made routinely. Intercalibrations on at least one sample have been made by all of the laboratories whose results are discussed below, so it appears that there are no major systematic errors.

The mineral phases that have proved most useful for these investigations are, (i) for the potassium-argon method: muscovite, biotite, phlogopite, sanidine, and glauconite; (ii) for the potassium-calcium method: sylvite; (iii) for the rubidium-strontium method: biotite, muscovite, microcline, and glauconite; and (iv) for the uranium-lead method: zircon, uraninite, pitchblende, and black shale. All of these mineral phases appear to be capable of remaining as closed chemical systems under specified conditions. The most sensitive, and therefore least useful, systems are potassium-calcium dating of sylvite and uranium-lead dating of black shales; in both these materials the criterion of a closed system is difficult to attain. The Pb^{207}–Pb^{206} age of zircon appears least susceptible to alteration. The common potassium micas and sanidine, if well crystallized, appear to retain their radiogenic products quantitatively if not heated to above 125° to 150°C. Microcline may retain its rubidium and strontium quantitatively at higher temperatures but leaks about 30 percent of its argon at room temperature. If well crystallized, glauconite may retain almost 100 percent of its argon for at least 100 million years, if maintained essentially at room temperature, but it loses its argon readily at elevated temperatures (*10*). Sylvite, when recrystallized, readily loses its radiogenic products.

There are specific geologic situations where each type of isotopic chronometer may be applied to the dating of sedimentary rocks. Volcanic ash beds intercalated with fossiliferous strata make possible precise stratigraphic definition. These ash beds, if uncontaminated by other sedimentary material, may carry zircon, biotite, and sanidine. If this material has not been seriously weathered or deeply buried, the potassium-argon ages determined with mica and sanidine and the uranium-lead ages determined with zircon should be accurate and concordant. Lavas which carry mica or zircon may be used similarly.

Table 2. Methods for determining the geochronometry of rock systems.

Isotopes	Minerals	Decay constants (yr^{-1})
U^{238}–Pb^{206}	Uraninite, pitchblende, zircon	1.54×10^{-10}
U^{235}–Pb^{207}	Uraninite, pitchblende, zircon	9.72×10^{-10}
Th^{232}–Pb^{208}	Uraninite, pitchblende, zircon	0.499×10^{-10}
Rb^{87}–Sr^{87}	Micas, potassium feldspars	1.47×10^{-11}
K^{40}–Ar^{40}	Micas, sanidine, glauconite	$\lambda_\beta = 4.72 \times 10^{-10}$
K^{40}–Ca^{40}	Sylvite	$\lambda_e = 0.584 \times 10^{-10}$

Igneous intrusives, such as granites, which cut one fossiliferous horizon and are unconformably overlain by another stratigraphically defined sedimentary rock may be dated by their micas (potassium-argon, rubidium-strontium), feldspar (rubidium-strontium), or zircon and uraninite (uranium-lead). Pitchblende veins may be used in the same way but are very sensitive to alteration and must be studied in some detail.

Glauconite is found in many sedimentary rocks and thus is an ideal chronometer from the viewpoint of stratigraphic definition. In any case, so long as it is well crystallized and carefully purified it will yield a reliable minimum age by the potassium-argon or rubidium-strontium methods (*15*). Likewise, many extrusive rocks as a whole retain 80 to 95 percent of their radiogenic argon, so that they also may be used to define minimum ages.

Sylvite is found in evaporite deposits of known stratigraphic level and may yield a valid K^{40}–Ca^{40} age determination under ideal conditions (*16*).

Cobb and I (*17*) have shown that, under ground-water conditions, lead loss is the common alteration pattern for the uranium mineral in black shales. Thus, these shales may yield valid minimum ages under certain conditions.

The problem of stratigraphic definition is twofold. For those cases where the chronometer is not formed at the time of formation of the fossiliferous unit, the geologic setting must define the total time interval. Even where the fossil association is immediate, the time uncertainty in intercontinental correlations may be larger than the analytical error in the age. Thus, although the age of a Tertiary ash fall might be determined to within 0.5 million years, the world-wide correlation of that particular faunal assemblage might be much less certain. Therefore, quantitative geochronometry has developed to the point where the isotopic ages may assist the paleontologist in determining absolute rates of evolution, ecological change,

and world-wide migration of organisms. The uncertainty in the isotopic ages, in years, increases from the Cenozoic to the Cambrian, but then so does the uncertainty in paleontological definition; hence, correlation of the new isotopic ages with the accepted intercontinental paleontological correlation should be valuable for all of Phanerozoic (post-Precambrian) time. For example, at the present time it is questionable whether synchronous fauna can be defined on a world-wide basis even for a 5- to 10-million-year interval such as Trenton time. Thus, the uncertainty in intercontinental correlation must be defined by the age measurement of the strata involved.

Critical Samples

Those samples that appear to be most definitive for the geological time scale are listed in Table 3 and described briefly in the appendix. For convenience they are divided by era. The precise geological data are essential in the use of these points. The errors listed in Table 3 are estimated analytical errors only, although it is believed that these samples represent closed chemical systems except where noted. From these data and certain supporting information from glauconites and effusive rocks, discussed below, it is possible to construct the geological time scale shown in Fig. 1.

1960 Geological Time Scale

Cenozoic. The first important division in the geological time scale is the boundary between the Recent and the Pleistocene. This is most logically placed at 11,000 years ago—a time marked by an abrupt change in sea level, sudden warming of the surface layer of the oceans, and rapid retreat of the continental glaciers (*18*, *19*). It should be noted that some outstanding students of the Pleistocene, such as Flint (*20*), believe that inclusion of the Recent epoch is superfluous. The beginning of the last major period of continental glaciation occurred about 70,000 years ago (*19*), and the substages of

Table 3. Critical points for the geological time scale.

Stratigraphic position	Locality	Rock	Mineral	Method	Age ($\times 10^6$ yr)	Reference
		I. Cenozoic Era				
Pleistocene-Pliocene boundary	Sierra Nevada, Calif.	Tuff	Biotite	K–Ar	1.0 ± 0.5	(*21*)
		Pliocene				
Latest Pliocene	Sutter Buttes, Calif.	Rhyolite	Biotite	K–Ar	1.7 ± 0.4	(*21*)
Upper Pliocene	Mendocino Co., Calif.	Sandstone	Glauconite	K–Ar	3.5 ± 0.5	(*15*)
Middle Pliocene	Kruisschens, Belgium	Sandstone	Glauconite	K–Ar	7 ± 1	(*22*)
Lower Hemphillian	Nevada	Tuff	Biotite	K–Ar	9.1 ± 0.5	(*31*)
Clarendonian	West Walker Canyon, Calif.	Welded tuff	Biotite	K–Ar	10.6 ± 0.5	(*31*)
Clarendonian	Nevada	Tuff	Biotite	K–Ar	11.1 ± 0.5	(*31*)
Clarendonian	Nevada	Tuff	Biotite	K–Ar	11.8 ± 0.5	(*31*)
Pliocene-Miocene boundary	Coal Valley, Nev.	Rhyolite tuff	Biotite	K–Ar	12.0 ± 0.5	(*31*)
		Miocene				
Barstovian	Barstow, Calif.	Dacite tuff	Biotite	K–Ar	15.2 ± 0.5	(*15*)
Barstovian	San Guillermo Quad., Calif.	Vitric tuff	Biotite	K–Ar	15.2 ± 0.5	(*15*)
Mid-Miocene	N. Cascades, Wash.	Snoqualmie granite	Biotite	K–Ar	17.0 ± 0.5	(*11*)
Hemingfordian	California	Tuff	Biotite	K–Ar	17.3 ± 0.6	(*31*)
Arikarean	Nebraska	Tuff	Biotite	K–Ar	21.6 ± 0.7	(*31*)
Lower Zemorrian	Cymric well No. 1, Calif.	Sandstone	Glauconite	K–Ar	23 ± 1	(*15*)
Burdigalian	Bad Hall, Austria	Sandstone	Glauconite	K–Ar	25 ± 1	(*15*)
		Oligocene				
Whitneyan	Oregon	Tuff	Biotite	K–Ar	25.7 ± 0.8	(*31*)
Chadronian	Texas	Tuff	Biotite	K–Ar	33.1 ± 1.0	(*31*)
		Eocene				
Upper Eocene	Vakis-Jvary, U.S.S.R.	Granite	Biotite	K–Ar	38 ± 4	(*27*)
Basal Kreyenhagen	Hernandez Valley, Calif.	Sandstone	Glauconite	K–Ar	43 ± 2	(*15*)
Lutetian	Fosse, Paris Basin	Sandstone	Glauconite	K–Ar	47 ± 2	(*15*)
Alpine	Kressenberg, Austria	Sandstone	Glauconite	K–Ar	51 ± 2	(*15*)
Upper Wilcox	Gulf Coast, Tex.	Sandstone	Glauconite	K–Ar	52 ± 2	(*15*)
Post early Eocene	Bearpaw Mt., Mont.	Carbonatite	Biotite	K–Ar	52 ± 2	(*13*)
Claiborne Group	Smithville, Tex.	Sandstone	Glauconite	K–Ar	54 ± 2	(*22*)
Lowermost Eocene	Clayton, N.J.	Sandstone	Glauconite	K–Ar; Rb–Sr	62 ± 2 55 ± 6	(*22*)
		Paleocene				
Pre-Eocene	Fakhralo, U.S.S.R.	Dacite	Biotite	K–Ar	57 ± 8	(*27*)
Top Paleocene	Gen. Petroleum Co. well, Calif.	Sandstone	Glauconite	K–Ar	59 ± 3	(*15*)
Upper Paleocene	Central City, Colo.	Ore	Pitchblende	U–Pb	59 ± 2	(*32*)
Mid-Paleocene	Oldhaven Group, England	Sandstone	Glauconite	K–Ar	57 ± 2	(*15*)
Paleocene	Standard Oil Co. well, Calif.	Sandstone	Biotite	K–Ar	59 ± 3	(*15*)
		II. Mesozoic Era				
		Cretaceous				
Uppermost Maestrichtian	Whitecourt, Alberta	Coal seam	Biotite	K–Ar	63 ± 2	(*11*)
			Sanidine	K–Ar	64 ± 2	
Maestrichtian	Strawberry Creek, Alberta	Tuff	Biotite	K–Ar	65 ± 2	(*11*)
			Sanidine	K–Ar	67 ± 2	
Maestrichtian	Gulf Coast	Sandstone	Glauconite	K–Ar	69 ± 2	(*15*)
Upper Campanian	Lethbridge, Alberta	Bentonite	Biotite	K–Ar	75 ± 2	(*11*)
			Sanidine	K–Ar	76 ± 2	
Lower Campanian	Hannover, Germany	Sandstone	Glauconite	K–Ar	81 ± 2	(*15*)

Stratigraphic position	Locality	Rock	Mineral	Method	Age ($\times 10^6$ yr)	Reference
Post Coniacian	Western Montana	Granite	Biotite	K–Ar	72–78	Appendix
Lower Santonian	Salzgitter, Germany	Sandstone	Glauconite	K–Ar	83 ± 3	(*15*)
Middle Turonian	Herne, Germany	Sandstone	Glauconite	K–Ar	83 ± 3	(*15*)
Lower Turonian	Dortmund, Germany	Sandstone	Glauconite	K–Ar	85 ± 3	(*15*)
Coniacian	Hannover, Germany	Sandstone	Glauconite	K–Ar	87 ± 3	(*15*)
Cenomanian	Coleman, Alberta	Agglomerate	Sanidine	K–Ar	93 ± 2	(*11*)
Cenomanian	Mill Creek, Alberta	Bentonite	Biotite	K–Ar	96 ± 3	(*11*)
			Sanidine	K–Ar	96 ± 3	
Turonian-Cenomanian	Coast Range, Calif.	Granites	Biotite	K–Ar	84–95	(*34*)
Upper Albian-Lower Cenomanian	Cache Creek, Calif.	Bentonite	Biotite	K–Ar	92 ± 3	(*15*)
Upper Albian	Lyme Regis, England	Sandstone	Glauconite	K–Ar	96 ± 3	(*15*)
Albian	Salzgitter, Germany	Sandstone	Glauconite	K–Ar	97 ± 3	(*15*)
Albian-Aptian	Salzgitter, Germany	Sandstone	Glauconite	K–Ar	102 ± 3	(*15*)
Upper Albian	Predkavkaz'e, U.S.S.R.	Sandstone	Glauconite	K–Ar	117 ± 12	(*23*)
Post Albian	So. Calif. batholith	Granite	Monazite	U, Th–Pb	116 ± 5	(*35*)
Middle Albian	Hudson Hope, B.C.	Tuff	Biotite	K–Ar	116 ± 3	(*11*)
Aptian	Gundelen, U.S.S.R.	Silt	Glauconite	K–Ar	118 ± 12	(*23*)
		Jurassic				
Post-Kimmeridgian–pre-Valanginian	Ono, Calif.	Granite	Biotite	K–Ar	127 ± 4	(*15*)
Portlandian	Moscow area, U.S.S.R.	Clayey sandstone	Glauconite	K–Ar	134 ± 13	(*23*)
Post-Kimmeridgian	Loomis, Calif.	Quartz diorite	Biotite	K–Ar	136 ± 4	(*15*)
Oxfordian	Oberpfalz, Germany	Sandstone	Glauconite	K–Ar	135 ± 4	(*15*)
Portlandian	Hannover, Germany	Sandstone	Glauconite	K–Ar	138 ± 4	(*15*)
Callovian	Oberpfalz, Germany	Shale	Glauconite	K–Ar	139 ± 4	(*15*)
Bathonian	Kelasury, Georgia	Granite	Biotite	K–Ar	165 ± 3	(*27*)
Lower Jurassic	Alaska	Granite	Biotite	K–Ar	169 ± 5	(*36*)
Post Triassic	Billiton, Indonesia	Granite	Biotite	K–Ar	180 ± 5	(*22*)
Triassic-Jurassic boundary	Ashcroft, B.C.	Granite	Biotite	K–Ar	181 ± 5	(*38*)
		Triassic				
Upper Triassic	Fort Lee, N.J.	Diabase	Biotite	K–Ar	195 ± 5	(*33*)
		III. Paleozoic Era				
		Permian				
Middle Permian	Solikamsk, U.S.S.R.	Evaporite	Sylvite	K–Ca	241 ± 8	(*16*)
Post mid-Autunian	Oslo, Norway	Granite	Biotite	K–Ar	259 ± 7	(*13*)
		Nordmarkite	Zircon	U–Pb	260 ± 5	
Carboniferous-Permian boundary	Dartmoor, England	Granite	Biotite	K–Ar, Rb–Sr	280 ± 5	(*8*)
			Uraninite	U–Pb	288 ± 5	(*40*)
		Carboniferous				
Uppermost Carboniferous	New South Wales, Australia	Toscanite	Biotite	K–Ar	287 ± 9	(*15*)
Tournaisian-Visean boundary	Vosges Mts., France	Granite	Biotite	K–Ar	315 ± 5	(*13*)
				Rb–Sr	322 ± 5	
Pre-Stephanian–post Visean	Harzburg, Harz	Gabbro	Biotite	K–Ar	325 ± 10	(*15*)
Post Lowermost Carboniferous	Magnitogorsk, U.S.S.R.	Granite	Biotite	K–Ar	340 ± 10	(*41*)
Devonian-Carboniferous boundary	Snobs Creek, Australia	Rhyolite	Biotite	K–Ar	344 ± 10	(*15*)
Devonian-Carboniferous boundary	Chattanooga fm., Tenn.	Bentonite; Black shale	Biotite	K–Ar	340 ± 7	(*13*)
				U–Pb	350 ± 15	(*17*)
		Devonian				
Post Lower Devonian	Jackman, Maine	Granite	Biotite	K–Ar, Rb–Sr	360 ± 5	(*12*)
Lower-Middle Devonian boundary	Eastern Greenland	Granite	Biotite	K–Ar	393 ± 10	(*44*)
Post-Silurian–pre-Upper Devonian	Eastern Maine	Granite	Biotite	K–Ar	404 ± 8	(*13*)
Lower Devonian	Shap, England	Granite	Biotite	K–Ar, Rb–Sr	395 ± 5	(*8*)
		Silurian				
Lower Silurian	Brassfield, Ohio	Sandstone	Glauconite	K–Ar	410 ± 15	(*22*)
		Ordovician				
Trenton	Bessemer, Ala.	Bentonite	Zircon	U^{238}–Pb^{206}	445 ± 10	(*13*)
Trenton	Eastern Tennessee	Bentonite	Zircon	U^{238}–Pb^{206}	447 ± 5	(*42*)
Upper Carodocian	Kinnekulle, Sweden	Bentonite	Sanidine	K–Ar	452	(*38*)
			Biotite	Rb–Sr	447	
Post Cambrian	Boisdale Hills, Nova Scotia	Granite	Biotite	K–Ar, Rb–Sr	490 ± 10	(*29*)
		Cambrian				
Post mid-Upper Cambrian	Shovde, Sweden	Black shale	Kolm	U–Pb	500 min.	(*17*)
Pre-basal Upper Cambrian	Wichita Mts., Okla.	Granite	Zircon	U–Pb	550 max.	(*28*)
Middle Cambrian	U.S.S.R.	Rhyolite	Whole rock	K–Ar	533 ± 50	(*43*)
Lower Cambrian	Kupa Station, U.S.S.R.		Glauconite	K–Ar	556 ± 56	(*23*)
Lower Cambrian	Lipyagi Village, U.S.S.R.		Glauconite	K–Ar	577 ± 58	(*23*)
Lower Cambrian	Serdobsk, U.S.S.R.		Glauconite	K–Ar	610 ± 61	(*23*)

this period are being defined in detail by radiocarbon dating. From the end of the Pliocene to 50,000 years ago, where the radiocarbon method becomes effective, there are very few data; but since Evernden (*21*) has reported the extension of the potassium-argon method down into the Pleistocene, it appears that in the next few years the subdivisions of the Pleistocene can be dated.

The date of the Pleistocene-Pliocene boundary is probably the most poorly known of the dates for boundaries between geologic periods, at least if expressed as the percentage of the correct age. From rather crude extrapolations of the rates of deep-sea sedimentation or of soil leaching and erosion over the past 70,000 years, the beginning of the Pleistocene can be placed at an estimated 500,000 to 2 million years before the present. Evernden (*21*) has reported dates on formations that are supposed to lie either at the Pliocene-Pleistocene boundary or in the late Pliocene which suggest an age of about 1 million years (Table 3), but the analytical error on these determinations was about 50 percent. Future work will undoubtedly permit close definition of the age of such rocks. The definition of synchronous faunas around the world within a time interval of only 100,000 years is so difficult that it may be best to use the beginning of the first continental glaciation, a global event, to define this boundary rather than try to define it by faunal assemblages, as is being done at present (*20*). With the existing data it appears best to assume a date of 1.0 million years before the present for the Pliocene-Pleistocene boundary, with the understanding that this figure is subject to considerable revision.

Great advances have been made in dating the Cenozoic epochs, largely as a result of the work of Curtis and Evernden at the University of California (*10, 15*). They have collected apparently uncontaminated volcanic ash beds, welded tuffs, and lava flows that are intercalated with continental vertebrate faunas of the western United States. The analyses of biotite from these rocks by the potassium-argon method should give highly reliable results, since the mica was not buried deeply or significantly heated subsequent to its formation. The only cause for uncertainty with such samples is the possibility that older detrital material was introduced, particularly in the case of the tuffs. Petrographic examination and multiple analyses on a given horizon should ultimately reduce this uncertainty to a very low value. These results are summarized in Table 3.

It also appears (*15, 22*) that glauconite can give a reliable age if it is found in a pure form, if it has been close to the surface throughout its history, and if it is less than 200 million years old. The small dimension of the crystal and the weakness of the bonding along the *c* axis make possible loss of argon under mild conditions. Evernden *et al.* (*15*), for example, found that at the relatively shallow depth of 8000 feet the well-formed glauconite from the Kreyenhagen formation of Upper Eocene age began to lose detectable amounts of argon. The potassium-argon age for glauconite is, therefore, always a minimum age, but under favorable circumstances glauconite may give the true age. Careful determinations of the potassium-argon age of glauconite, taken throughout the geological column, have been made by Kazakov and Polevaya (*23*), Hurley *et al.* (*22*), and Evernden *et al.* (*15*). Where any of these glauconite determinations are useful in setting a minimum date for an epoch or stage, they have been included in Table 3. It appears that quite a number of post-Triassic glauconites have retained most, if not all, of their argon and therefore give isotopic ages in agreement with those obtained on other, more stable minerals. Before the Mesozoic, however, the glauconite ages are always lower than the probable age. Hurley *et al.* (*22*) suggest this may be due to the gradual addition of potassium to the glauconite structure. An alternative view is that the surface temperature is adequate to cause detectable loss of argon by diffusion. For example, if the average of the highest glauconite ages for each epoch is taken, the data are roughly consistent with an argon loss of 2 to 3 percent per 100 million years. In any event, the glauconite data provide useful minimum dates.

There are eight well-defined points in the Pliocene, suggesting that the Hemphillian stage began about 10 million years ago and that the base of the Pliocene should probably be taken as 13 ± 1 million years.

In the Miocene, dates are available for the Barstovian, Hemingfordian, Arikarean, and Burdigalian stages. Age determinations for biotite from the Snoqualmie granite and for glauconite from California and Austria fit consistently with determinations for ashbed samples. From the data on the Whitneyan (uppermost continental stage of the Oligocene), the Miocene-Oligocene boundary is dated at 25 ± 1 million years. Evernden *et al.* (*15*), using only their data, suggest 26 million years for this boundary.

Only two dates are available in the Oligocene, but from ages determined for the Upper Eocene granite from Vakis-Jvary, U.S.S.R. (38 ± 4 million years) and for the Kreyenhagen glauconite (43 ± 2 million years) (basal Upper Eocene), the Oligocene-Eocene boundary is dated at 36 ± 2 million years.

In the Eocene it is noteworthy that the Upper Wilcox glauconite provides a minimum date of 52 ± 2 million years for the top of the Lower Eocene, and that 52 ± 2 million years is also obtained for the age of the biotite from the Bearpaw Mountain carbonatite, which must have been intruded *after* the Wasatch stage of the early Eocene (*13, 24*). The Upper-Middle boundary of the Eocene is tentatively dated at 45 ± 2 million years, and the Middle-Lower Eocene boundary, at 52 ± 2 million years.

A number of samples bear on the date of the Eocene-Paleocene boundary. The Central City pitchblende, which is post-Fort Union (*25*), and the glauconite from a General Petroleum Company well in California both give ages of 59 million years and represent uppermost Paleocene. The Lower Eocene glauconites give average ages of 54 and 59 million years, with errors of about ± 2 million years. It appears that the Eocene-Paleocene boundary dates back 58 ± 2 million years.

Mesozoic. The excellent determinations by Folinsbee and his co-workers (*11*) of the age of the uppermost Maestrichtian, made by concordant potassium-argon age determinations on biotite and sanidine, fix the end of the Cretaceous at 63 ± 2 million years before the present. This date indicates that the Danian stage is Paleocene, but the analytical error is probably greater than the stratigraphic uncertainty. The date of 70 million years taken by Evernden *et al.* (*15*) for the Danian-Maestrichtian boundary appears too early in view of these new data.

The concordant dates given by two minerals from various Cretaceous ash beds in determinations by Folinsbee and his co-workers (*11*) produce the most precisely dated interval in the time scale. In addition, there are a number

of useful glauconite dates for the Cretaceous. Collectively these data suggest the following boundary dates, in millions of years before the present: Maestrichtian-Campanian, 72 ± 3; Campanian-Santonian, 81 ± 3; Santonian-Coniacian, 84 ± 3; Coniacian-Turonian, 88 ± 3; Turonian-Cenomanian, 90 ± 2; Cenomanian-Albian, 110 ± 3; and Albian-Aptian, 120 ± 4. The Crowsnest volcanics (Coleman, Alberta, sample) are underlain by strata containing late Albian flora. They are interbedded with strata containing late Albian or early Cenomanian plants, which are in turn overlain by marine shales of upper Cenomanian age. According to Folinsbee (*26*), 100 feet above the Crowsnest zone the worldwide *Inoceramus labiatus* zone occurs (Lower Turonian).

There are too few data to permit a subdivision of the Cretaceous below the Albian-Aptian boundary.

The Cretaceous-Jurassic boundary must be defined from the minimum glauconite date of the Portlandian (Upper Jurassic) sample from Hannover, Germany, and from the Ono, California, granite, which must have been intruded between the end of the Kimmeridgian (Upper Jurassic) and the beginning of the Valanginian (Lower Cretaceous). Hence, the Valanginian probably dates back less than 127 ± 4 million years, and the Portlandian, probably at least 138 ± 4 million years. The Loomis, California, sample shows that the top of the Kimmeridgian must be at least 136 ± 4 million years old. The glauconite sample from the Portlandian of the Moscow area will be more useful when the experimental error is reduced. Consideration of all these data leads to the suggestion of 135 ± 5 million years before the present as the date of the Cretaceous-Jurassic boundary.

The Bathonian is well dated at 165 ± 2 million years, both by Rubinstein (*27*) and by workers at this laboratory. The date of the Jurassic-Triassic boundary appears to be 181 ± 5 million years, to judge from three biotite ages of well-defined granites which lie very close to this boundary. The biotite age on the Palisades diabase of the Newark series of the Upper Triassic suggests that the date of the Middle-Upper Triassic boundary is probably about 200 ± 10 million years. There are no data by which to date, in years, the other subdivisions of the Triassic. By linear interpolation the Triassic-Permian boundary is set at about 230 million years, but the uncertainty is probably at least ± 10 million years.

Paleozoic. The sylvite dated as 241 ± 8 million years by Polevaya *et al.* (*16*) by the potassium-calcium method is known to be Kungurian (Middle Permian). The experimental error is that given by the analysts and is dominated by the correction for common calcium in the sample. Since sylvite can recrystallize easily, this should probably be regarded as a minimum date, though Polevaya *et al.* (*16*) claim the sample was not recrystallized.

Since the Oslo igneous series was dated by Faul (*13*) as probably late Lower Permian, a date of 260 ± 10 million years for the Middle-Lower Permian boundary seems reasonable. The Dartmoor granite and associated veins have been dated by potassium-argon, rubidium-strontium, and uranium-lead determinations on various minerals by various laboratories. The average ages given by these methods are quite similar, suggesting a time of intrusion of 280 ± 5 million years before the present. The most recent evaluation of the stratigraphic relationships (*9*) suggests that the granites are certainly post-Lower Stephanian and pre-Middle Permian. In view of the toscanite in New South Wales, which is interlayered in uppermost Carboniferous rocks, the Carboniferous-Permian boundary is tentatively set at 280 ± 10 million years.

The intensive study by Faul (*13*) on the micas from the Vosges granites, which were intruded near the Tournaisian-Visean boundary, gives concordant ages, by the rubidium-strontium and potassium-argon methods, of 320 ± 5 million years. The single determination on the biotite from the gabbro of the Harz Mountains in Germany is not inconsistent with this, if the fairly large analytical error is considered. No further subdivision of the Carboniferous is currently possible.

The Carboniferous-Devonian boundary, however, appears to be rather well defined, in part due to the occurrence of both a uranium-rich black shale and a volcanic ash bed in the Chattanooga formation of Tennessee, which is thought to lie astride the boundary that separates these two periods. There is some debate among stratigraphers as to whether this formation should be placed wholly in the uppermost Devonian or partly in the basal Carboniferous, but this question is probably trivial insofar as the age is concerned, in view of the present degree of analytical precision. In addition, this boundary is bracketed by the Magnitogorsk granite, which cuts lowermost Carboniferous beds, and the Snobs Creek rhyolite, which is either uppermost Devonian or at the Devonian-Carboniferous boundary. When all of these data are taken into account, the best date for the Devonian-Carboniferous boundary would appear to be 345 ± 10 million years.

The highly consistent results on intrusives and contact metamorphic rocks from the Jackman, Maine, area obtained by Hurley *et al.* (*12*) provide an analytically accurate date of 360 ± 5 million years. Unfortunately, the stratigraphic relations only permit this material to be identified as post-Oriskany (that is, post-Lower Devonian). Thus, although the material appears to be Upper Devonian, it does not provide a close definition. The other three Devonian rocks do provide dates that rather closely define the Lower Devonian-Middle Devonian boundary. The sample from eastern Greenland lies either at the Lower-Middle Devonian boundary or slightly up into the Middle Devonian. The granites from eastern Maine are post-Silurian and probably post-lowest Devonian. Although it is only certain that they are pre-Upper Devonian, it is probable that they were formed immediately after the post-Silurian folding. The Shap granite has been dated by the Oxford, Harwell, and Lamont laboratories from determinations on multiple samples, which give an average age of 395 ± 5 million years. This granite, too, must lie somewhere up in the Lower Devonian. The Lower-Middle Devonian boundary, therefore, would seem to be dated about 390 ± 5 million years ago. The date 405 ± 10 million years has therefore been chosen rather arbitrarily for the base of the Devonian.

Dates in the next group are all from Ordovician rocks of the Trentonian in North America (Alabama and Tennessee) and the upper Carodocian in Sweden. From determinations on biotite and zircon from ash beds, the age appears to be close to 445 million years. If this date is accepted for Trenton, then the Ordovician-Silurian boundary would be at about 425 ± 10 million years. There is currently no information to permit subdivision of the other stages of the Ordovician.

The date for the Upper Cambrian-Ordovician boundary and the duration of the Upper Cambrian can be roughly inferred from the results of three sepa-

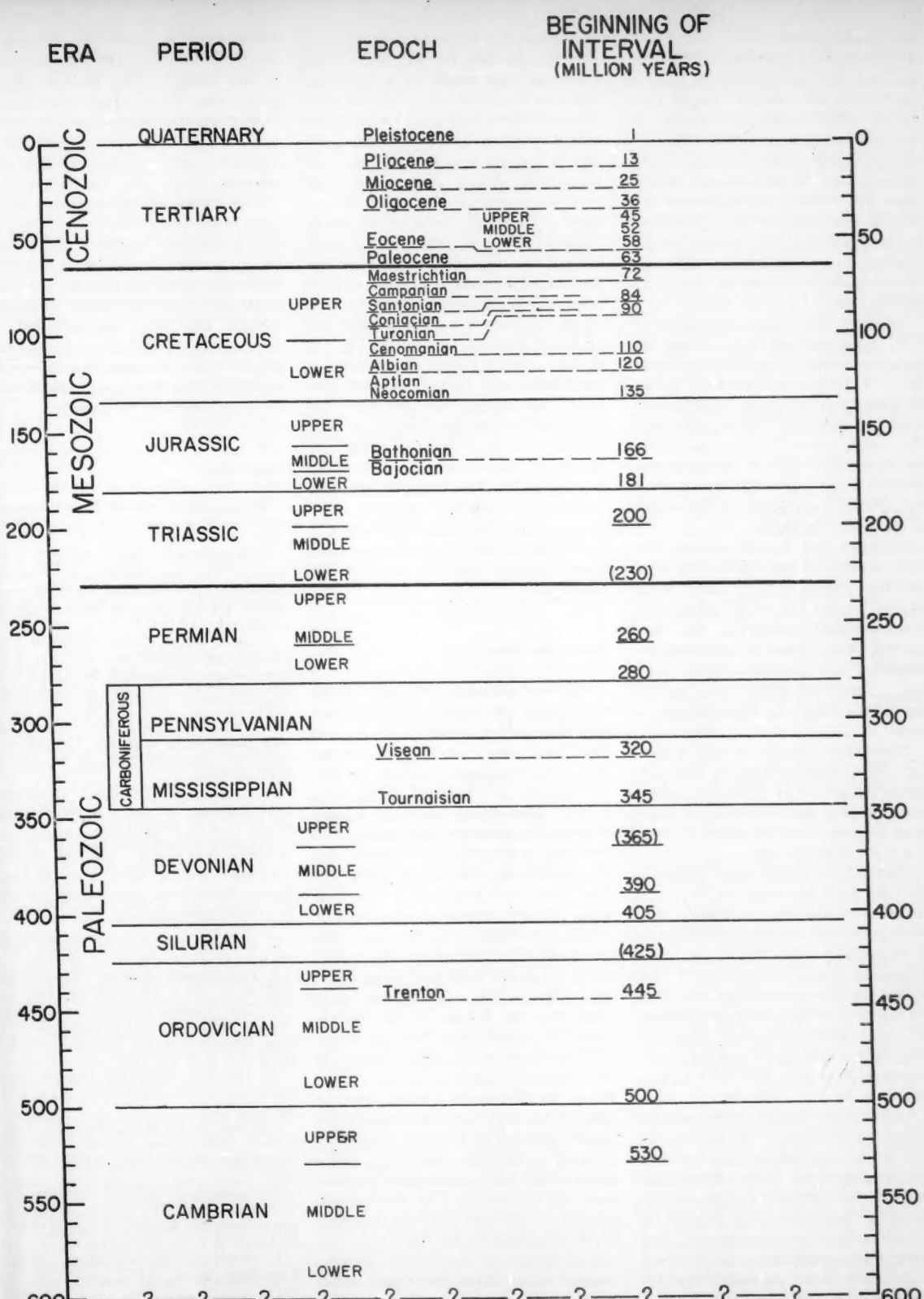

Fig. 1. Geologic time scale.

rate investigations. Cobb and I (*17*) have studied the geochemistry of uranium and lead in the Swedish kolm of the Peltura zone (middle Upper Cambrian). This study showed that the alteration process has consisted of lead loss, including preferential loss of lead-206, so that the highest age obtained from U^{235}–Pb^{207} determinations is a reliable minimum for this material. It is concluded that 500 million years is a minimum age for this formation and that it probably is not as old as 550 million years.

On the other hand, Aldrich *et al.* (*28*) have analyzed zircon from the igneous complex in the Wichita Mountains of Oklahoma, which is unconformably overlain by the Reagan sandstone of the basal Upper Cambrian. The nearly concordant results gave an age, from Pb^{207}–Pb^{206} determinations, of 550 million years. This should represent a reliable maximum for the bottom of the Upper Cambrian.

Fairbairn and his co-workers (*29*) have determined the age of the Boisdale Hills granite in Nova Scotia, which cuts uppermost Cambrian and possibly cuts lowermost Ordovician. The average age, as determined by the rubidium-strontium and potassium-argon methods, is about 485 million years, but the deviation among the measurements is large.

These three groups of data suggest that the Cambro-Ordovician boundary should be placed at 500 ± 10 million years and that the Middle-Upper Cambrian boundary may be placed at about 530 ± 15 million years.

For the Middle and Lower Cambrian, the only available dates are on whole extrusive rocks and glauconite. The analytical errors are large, and the exact relationship to *Olenellus* of the rocks designated Lower Cambrian is not known. If it is assumed, however, that the Lower Cambrian beds are correlative with beds containing *Olenellus*, these data would suggest that the Lower Cambrian is at least 580 ± 20 million years old.

An important feature of the determinations on glauconite on the Russian platform (*30*) is that more or less continuous ages, from about 600 million years to 1300 million years, have been found for glauconite lying directly on the 1450-million-year-old basement. The stratigraphic identification of the rocks from which these glauconite samples were taken ranges from Lower Cambrian to Sinian. Thus, if it is assumed that argon loss averaged only about 10 percent, the date for the base of the *Olenellus* zone would lie in the range 600 to 650 million years. On the other hand, further experimental work is required on these materials before accurate values are available. If the Cambrian lasted as long as the usual major geologic period, as the depth to the base of *Olenellus*-bearing strata would indicate, then *Olenellus* almost surely entered the scene later than 600 million years ago. In order, therefore, to set a reasonably accurate date for the base of the Cambrian, more measurements on better materials are needed. If it is decided that the first appearance of *Olenellus* marks the base of the Cambrian, the question of date may not be settled for decades, while all parts of the world are being examined to determine where and when *Olenellus* first appeared. If it should be definitely shown that *Olenellus* appeared more recently than 600 million years ago, it might then be best simply to adopt this round number for the base of the Cambrian.

New Time Scale

The new geological time scale developed from the critical determinations described above is shown in Fig. 1. The linear time scale is given at the left. The boundaries between periods and epochs are indicated where data permit. Parentheses are used if considerable interpolation was required.

It may be seen that the Cenozoic and Cretaceous are now much better defined than any other part of the time scale. Much remains to be done in defining the Pleistocene. No data exist for the Silurian, and only a few quantitative points are available for the Cambrian, Ordovician, and Triassic. It is clear that the Eocene is the longest epoch (18 million years) and the Paleocene the shortest (4 million years) in the Tertiary. Likewise, in the Cretaceous, the Cenomanian is the longest stage, occupying nearly one-third of the period, whereas the Danian, Santonian, Coniacian, Turonian, Barremian, Hauterivian, and Valanginian must be quite short, on the order of a few million years each (the last three are included in the Neocomian of Fig. 1).

The length of the major periods averages about 60 million years, with surprising uniformity. The longest period, except for the Cambrian, about which there is uncertainty, appears to be the Ordovician. The shortest period is the Silurian. The division of the periods into Upper (late), Middle, and Lower (early), clearly does not, on the basis of biostratigraphic considerations, represent equal or even similar time intervals.

The geological time scale has developed to the place where it can be used for correlation problems in paleontology, orogeny, and mineralization. Although much remains to be done, particularly in the lower Paleozoic, enough has been accomplished to demonstrate the potential accuracy that may ultimately be attained and the new objectivity that has been introduced in the time dimension in geology.

Appendix

Description of critical samples used in constructing the time scale for the Cenozoic.

1. Bishop tuff, Sierra Nevada, California. This tuff lies beneath a widely distributed moraine system but still overlies glacial till of unknown affinity. K–Ar, biotite age, $(1.0 \pm 0.5)\ 10^6$ yr (*21*).

2. Sutter Buttes, California. Biotite from rhyolite and andesite from the latest Pliocene volcanic unit gave K–Ar, $(1.5 \pm 0.5)\ 10^6$ and $(1.8 \pm 0.5)\ 10^6$ yr, respectively (*21*).

3. Mendocino County, California. Surface glauconite in sandstone. Unweathered material. Upper Pliocene. K–Ar, $(3.5 \pm 0.5)\ 10^6$ yr (*15*).

4. Kruisschens, Belgium. Glauconite from Scaldisien formation in the Pliocene. K–Ar, $(7 \pm 1)\ 10^6$ yr (*15*).

5. Lower Hemphillian, Nevada. Biotite from tuff. K–Ar, $(9.1 \pm 0.5)\ 10^6$ yr (*31*).

6. West Walker River Canyon, California. Biotite from latite welded tuff that is Clarendonian in age. K–Ar, $(10.6 \pm 0.5)\ 10^6$ yr (*31*).

7. Clarendonian, Nevada. Biotite from tuff. K–Ar, $(11.1 \pm 0.5)\ 10^6$ yr (*31*).

8. Clarendonian, Nevada. Biotite from tuff. K–Ar, $(11.8 \pm 0.5)\ 10^6$ yr (*31*).

9. Coal Valley, Nevada. Biotite from rhyolite tuff. Lower Clarendonian very near Miocene-Pliocene boundary. K–Ar, $(12.0 \pm 0.5)\ 10^6$ yr (*15*).

10. Barstow, California. Biotite from dacite tuff, Barstovian. K–Ar, $(15.2 \pm 0.5)\ 10^6$ yr (*15*).

11. San Guillermo Quadrangle, California. Biotite from crystal vitric tuff, Barstovian. K–Ar, $(15.2 \pm 0.5)\ 10^6$ yr (*15*).

12. Snoqualmie batholith intrudes late Oligocene (John Day vertebrate fauna) and is generally accepted as mid-Miocene. K–Ar, $(17.0 \pm 0.5)\ 10^6$ yr (*11*).

13. Hemingfordian, California. Biotite from tuff. K–Ar, $(17.3 \pm 0.6)\ 10^6$ yr (*31*).

14. Arikarean, Nebraska. Biotite from tuff. K–Ar, $(21.6 \pm 0.7)\ 10^6$ yr (*31*).

15. 22S-21E Hub Petroleum cymric well No. 1, California. Glauconite of Lower Miocene (Lower Zemorrian); (23 ± 1) 10^6 yr (*15*).

16. Bad Hall, Austria. Glauconite from Lower Miocene (Burdigalian). K–Ar, (25 ± 1) 10^6 yr (*15*).

17. Whitneyan, Oregon. Biotite in tuff. K–Ar, (25.7 ± 0.8) 10^6 yr (*31*).

18. Chadronian, Texas. Biotite in tuff. K–Ar, (33.1 ± 1.0) 10^6 yr (*31*).

19. Biotite from a granite at Vakis-Jvary, Georgia, U.S.S.R., which cuts fossiliferous Middle Eocene but which is overlain by fossiliferous Lower Oligocene. Geologists believe the intrusion took place in the lower part of the Upper Eocene. K–Ar, (38 ± 4) 10^6 yr (*27*).

20. Hernandez Valley, California. Excellent surface glauconite from Basal Kreyenhagen (base of Upper Eocene). K–Ar, (45 ± 2) 10^6 yr (*15*).

21. Fosse, Paris Basin. Excellent glauconite from surface outcrop, Lutetian stage of Middle Eocene. K–Ar, (47 ± 2) 10^6 yr (*15*).

22. Kressenberg, Austria. Glauconite from Lower Eocene. K–Ar, (51 ± 2) 10^6 yr (*15*).

23. Gulf Coast. Glauconite from upper Lower Eocene (Upper Wilcox). K–Ar, (52 ± 2) 10^6 yr (*15*).

24. Biotite from carbonatite in Bearpaw Mountains, Montana. Post-early Eocene. K–Ar, (52 ± 2) 10^6 yr (*13*).

25. Smithville, Texas. Glauconite from Viesca, Weches formation in Eocene. K–Ar, (54 ± 2) 10^6 yr (*22*).

26. Clayton, N.J. Glauconite from Hornerstown formation in Lower Eocene. K–Ar, (62 ± 2) 10^6 yr; Rb–Sr, (55 ± 6) 10^6 yr (*22*).

27. Fakhralo, U.S.S.R. Biotite in dacites of post-Turonian but pre-Eocene age. K–Ar, (57 ± 8) 10^6 yr (*27*).

28. Kodor River, Abkhaziya, U.S.S.R. Glauconite from limestone with *Protocardium Edwardsi Desh.* var. *orientalis, Spondylus* sp., *area* sp., *Dentalium* sp., and so on. Upper Paleocene. K–Ar, (58 ± 6) 10^6 yr (*23*).

29. General Petroleum Espe Road 75–33, California. Glauconite from top of Paleocene. K–Ar, (61 ± 2) 10^6 yr (*15*).

30. Pitchblende from Central City, Colorado. Stratigraphic age is post-Fort Union (Upper Paleocene) but pre-Eocene (*25*); (59 ± 3) 10^6 yr, based on the average of several U^{238}–Pb^{206} ages (*32*).

31. Standard Oil Co., well No. 62-13C, California. Biotite of volcanic origin from glauconitic sandstone, Paleocene in age. K–Ar, (59 ± 3) 10^6 yr (*15*).

Description of critical samples used in constructing the time scale for the Mesozoic.

32. Pembina coal seam, Whitecourt, Alberta, 54°05′N, 115°31′W. Biotite (63×10^6 yr) and sanidine (K–Ar, 64×10^6 yr) from uppermost Maestrichtian (*11*).

33. Strawberry Creek, Alberta, 53°16′N, 114°07′W. Sanidine [(67 ± 2) 10^6 yr, three samples] and biotite [(65 ± 2) 10^6 yr, two samples] from Maestrichtian. K–Ar, (66 ± 1) 10^6 yr (average) (*11*).

34. Gulf Coast. Glauconite from sandstone outcrop from the Ripley formation, Maestrichtian stage. K–Ar, 68.5×10^6 yr, 69.5×10^6 yr (*15*).

35. Lethbridge, Alberta, 49°42′N, 114° 31′W. Biotite [(75 ± 2) 10^6 yr] and sanidine [(76 ± 2) 10^6 yr] from bentonite No. 1 in Bearpaw shale (Upper Campanian). K–Ar, (75 ± 2) 10^6 yr (*11*).

36. Western Montana, Boulder batholith. Various phases have been dated by K–Ar on biotite. Folinsbee reported 74×10^6 yr, Curtis 74×10^6 yr, and Kulp 72×10^6 yr at the New York Academy of Sciences Conference (see *11, 31, 33*).

37. Hannover, Germany. Core from 500 meters in glauconitic sandstone of Lower Campanian stage. K–Ar, (81 ± 2) 10^6 yr (*15*).

38. Salzgitter, Germany. Glauconite from sandstone of Lower Santonian age. K–Ar, (83 ± 3) 10^6 yr (*15*).

39. Herne, Germany. Glauconite from sandstone in Bochumer Greensand of the Middle Turonian stage of the Upper Cretaceous. K–Ar, (83 ± 3) 10^6 yr (*15*).

40. Dortmund, Germany. Core from well. Glauconitic sandstone of Lower Turonian age. K–Ar, (85 ± 3) 10^6 yr (*15*).

41. Hannover, Germany. Core from 711 meters in glauconitic sandstone of Coniacian stage. K–Ar, (87 ± 3) 10^6 yr (*15*).

42. Coleman, Alberta, 49° 39′N, 114° 31′W. Sanidine from Crowsnest volcanic agglomerate. Cenomanian. K–Ar, (93 ± 2) 10^6 yr (*11*).

43. Mill Creek, Alberta, 49°25′N, 114° 09′W. Biotite and sanidine from bentonite. Cenomanian. K–Ar, (94 ± 2) 10^6 yr (*11*).

44. Coast Range granites and granodiorites of California. Turonian-Cenomanian. K–Ar, 84×10^6 to 95×10^6 yr (*34*).

45. Cache Creek, California. Biotite from bentonite in top of Antelope Shale (Upper Albian or Lower Cenomanian). K–Ar, (92 ± 3) 10^6 yr (*15*).

46. Lyme Regis, England. Glauconite from outcrop of Upper Greensand of Dorset Coast. K–Ar, (101 ± 3) 10^6 yr (*15*).

47. Salzgitter, Germany. Core of glauconitic sandstone from 430 meters, Albian stage. K–Ar, (97 ± 3) 10^6 yr (*15*).

48. Salzgitter, Germany. Core of glauconitic sandstone from 613 meters, Albian-Aptian boundary. K–Ar, (102 ± 3) 10^6 yr (*15*).

49. Predkavkaz'e, U.S.S.R. Glauconite from clayey sandstones in the Vyselki orientation borehole (Upper Albian). K–Ar, (117 ± 12) 10^6 yr (*23*).

50. Southern California batholith. Monazite from granite which invades and metamorphoses fossiliferous limestones and volcanic sediments of Albian age. The whole sequence is overlain by sedimentary rocks of Maestrichtian age. Th, U–Pb, (115 ± 5) 10^6 yr (*35*).

51. Baksan River, Gundelen, Caucasus, U.S.S.R. Glauconite from silts of Aptian age. K–Ar, (118 ± 12) 10^6 yr (*23*).

52. Hudson Hope, British Columbia. Biotite from tuff in Harmon shale (Middle Albian). K–Ar, (116 ± 3) 10^6 yr (*11*).

53. Ono, Shasta County, California. Biotite from quartz diorite of the Shasta Valley batholith. By association with other intrusives this has been designated post-Kimmeridgian and pre-Valanginian. Evernden *et al.* (*15*) consider this to be close to the Jurassic-Cretaceous boundary. K–Ar, (127 ± 4) 10^6 yr (*15*).

54. Moscow area, U.S.S.R. Egorevskoe deposit. Horizon under the phosphorites: *Oxynoticeras fulgens* fauna. Glauconite in clayey sands. Upper Jurassic (Volga stage, Portlandian). K–Ar, (134 ± 13) 10^6 yr (*23*).

55. Loomis, California. Biotite from quartz diorite from quarry near Horseshoe Bar on the American River 3 miles northeast of Loomis. K–Ar, (136 ± 4) 10^6 yr (*15*).

56. Oberpfalz, Germany. Core from 29.4 meters in glauconitic sandstone. Oxfordian stage (Plicatilis Schichten). K–Ar, (135 ± 4) 10^6 yr (*15*).

57. Hannover, Germany. Core from 24 meters in glauconitic sandstone. Upper Portland stage (Munder Mergel). K–Ar, (138 ± 4) 10^6 yr (*15*).

58. Oberpfalz, Germany. Core from 33 meters in shale containing glauconite pellets representing Callovian stage of Upper Jurassic (Ornatentone). K–Ar, (139 ± 4) 10^6 yr (*15*).

59. Kelasury, Georgia, U.S.S.R. Biotite in intrusive that cuts Bajocian is pre-Lower Cretaceous and probably Bathonian. K–Ar, (165 ± 3) 10^6 yr (*27*).

60. Alaska. Pre-Pliensbachian but Lower Jurassic. K–Ar, (169 ± 5) 10^6 yr (*36*).

61. Billiton, Indonesia. Tin granite. Latest Triassic or early Lower-Jurassic biotite. K–Ar, (180 ± 5) 10^6 yr (*37*).

62. Ashcroft, British Columbia. Biotite from granite in the Guichon batholith. Post-Carnian, pre-Bajocian. K–Ar, 181×10^6 yr (*38*).

63. Fort Lee, N.J. Biotite from the Palisades sill. Associated with lavas in the Newark series of the Upper Triassic. Probably early or middle Upper Triassic; (193 ± 3) 10^6 yr (*33*).

Description of critical samples used in constructing the time scale for the Paleozoic.

64. Solikamsk, U.S.S.R. Bed "B" of the Verkhnekamerisk formation (Mt. Berezniki), Solikamsk. This lies in the Kungar (Middle Permian) stage. Polevaya *et al.* (*16*) believe this sample of coarse crystals of milky white sylvite was not recrystallized. The K^{40}–Ca^{40} age is (241 ± 8) 10^6 yr, whereas the K^{40}–Ar^{40} age is lower [(224 ± 10) 10^6 yr], indicating some argon loss. The K–Ca age is adopted.

65. Oslo, Norway. Granite and nordmarkite intrude Oslo porphyry lavas which are interbedded with fossiliferous lake sediments of early Permian age correlative with the German *Rotliegendes*. O. A. Hoeg believes that the lake sediments probably date from about the middle of the Lower Permian. [Work done by Faul *et al.* in 1959 (*39*).] Biotite from the Drammen granite; K–Ar, 259×10^6 yr. Zircon from the Oslo nordmarkite; U^{238}–Pb^{206}, (259 ± 5) 10^6 yr.

66. Dartmoor, England. Granite which cuts folded Westphalian, where folds are aligned with those that involve Lower

Stephanian strata. Overlain by Middle Permian gravels but probably near Permian-Carboniferous boundary. Many K–Ar and Rb–Sr measurements on a variety of samples give an average value of (280 ± 5) 10^6 yr (*8*).

67. Cornwall, England. Uraninite from Geevor Mine, which is probably associated with Dartmoor type granite. U^{238}–Pb^{206}, 288 × 10^6 yr; U^{235}–Pb^{207}, 288 × 10^6 yr; Pb^{207}/Pb^{206}, (307 ± 25) 10^6 yr; Th^{232}/Pb^{208}, (300 ± 15) 10^6 yr (*40*).

68. Lower Hunter Valley, South Wales, Australia. Biotite from toscanite in the Kuttung Series. K–Ar, (287 ± 9) 10^6 yr (*15*). These beds were deposited before the Permian glacial period and are directly overlain by lower Permian marine beds.

69. Vosges Mountains, northeast Europe. Here granites have intruded and metamorphosed Tournaisian sedimentary rocks, and they are overlain by fossiliferous strata of early Visean, so this formation closely marks the Visean-Tournaisian boundary. Three samples of biotite analyzed by the Rb–Sr method gave an average isotopic age of (322 ± 5) 10^6 yr. Four samples of biotite analyzed by the K–Ar method gave an average isotopic age of (315 ± 5) 10^6 yr (*13*).

70. Harzburg, Harz, Germany. Biotite separated from the Harzburger gabbro, which is pre-Stephanian but post-Kulm-graywacke (probably Visean). K–Ar, (325 ± 10) 10^6 yr (*15*).

71. Magnitogorsk, U.S.S.R. Biotite from granite which cuts a thin unit of lowermost Carboniferous. Presumably this cut occurred shortly after deposition. K–Ar, (340 ± 10) 10^6 yr (*41*).

72. Snobs Creek, Warburton, Victoria, Australia. Biotite from rhyolite sampled 2 miles east of the Rubicon power station. Essentially at the top of the Devonian. K–Ar, (341 ± 10) 10^6 yr (*15*).

73. Smithville, Tennessee. Biotite from bentonite layer in Dowelltown, member of the Chattanooga shale taken at Sligo Bridge. K–Ar, (340 ± 10) 10^6 yr (*13*).

74. Youngs Bend area, Tennessee. Whole-rock analyses by the U^{238}–Pb^{206} method gave (350 ± 15) 10^6 yr; this is theoretically the minimum age, but in this case of relatively uniform uranium distribution it may be close to the true age (*17*).

75. Jackman, Maine. Hog Island quartz monzonite intrudes and has metamorphosed fossiliferous rock of Oriskany age; hence the intrusion is post-Lower Devonian. Rubidium-strontium dating on biotite from monzonite gave 362 × 10^6 yr; four samples from granite dated by K–Ar gave 360 × 10^6 yr. Four other samples, from whole slate and hornfels, gave 365 × 10^6 yr (*12*).

76. Eastern Greenland geosyncline. Granite which intrudes Lower Devonian and is overlain by Middle Devonian. K–Ar, (393 ± 10) 10^6 yr (*14*).

77. Shap, Westmorland, England. Granites that were intruded after the Silurian folding and probably after the moderate folding of lowest Devonian. Considered Downtownian, which is the continental equivalent of Maine Lower Devonian (*9*). Four separate determinations of K–Ar ages at the Lamont laboratory gave (391 ± 7) 10^6 yr. Four separate Rb–Sr determinations at Oxford gave (397 ± 11) 10^6 yr. Four Rb-Sr determinations at Harwell gave (372 ± 20) 10^6 yr (*8*).

78. Brassfield, Ohio. Glauconite from sandstone in Lower Silurian. K–Ar, (410 ± 15) 10^6 yr (*22*).

79. Bessemer, Alabama. Zircon from bentonite of the Carters limestone analyzed by G. R. Tilton. U^{238}–Pb^{206}, (445 ± 10) 10^6 yr (*13*).

80. Eastern Tennessee and Alabama. Zircon from the Ordovician bentonites of Trenton age (the Stones River group in Tennessee and the Bays group in Alabama) gave (447 ± 10) 10^6 yr for the U^{238}–Pb^{206} age. Rubidium-strontium ages on biotite from these horizons are reported to be consistent with the U^{238}–Pb^{206} age (*41*).

81. Kinnekulle, Sweden. Sanidine and biotite from the upper Caradocian horizon. Three determinations on biotite gave 447 × 10^6 yr, and two on sanidine gave 452 × 10^6 yr, by the K–Ar method (*26*).

82. Boisdale Hills, Nova Scotia. Biotite from granite which clearly cuts uppermost Cambrian and possibly some Lower Ordovician. Both K–Ar and Rb–Sr analyses gave about 485 × 10^6 yr (*29*).

83. Shovde, Sweden. In a study of the geochemistry of uranium and lead in the kolm of this locality it was shown that the age of 500 × 10^6 yr for this formation is a reliable minimum (*17*).

84. Wichita Mountains, Oklahoma. Zircon from granite. According to the geologists who have studied the area (W. Hamilton and W. H. Ham and their associates), the rock from which the zircon was taken is clearly part of the igneous complex which is uncomformably overlain by the basal Upper Cambrian Reagan sandstone. The ages were as follows: U^{238}–Pb^{206}, 520 × 10^6 yr; U^{235}–Pb^{207}, 525 × 10^6 yr; and Pb^{207}–Pb^{206}, 550 × 10^6 yr. Thus, the Reagan is probably younger than 550 × 10^6 yr.

85. U.S.S.R. Whole-rock analysis on felsite intercalated in Middle Cambrian rocks. K–Ar, (533 ± 50) 10^6 yr (*43*).

86. Kupa Station, Belorussia, U.S.S.R. Borehole R-2, 252 to 257 meters deep. Glauconite from Lower Cambrian limestone. K–Ar, (556 ± 56) 10^6 yr (*23*).

87. Lipyagi Village, U.S.S.R. Glauconite from standstone from borehole R-1, 1430 meters deep. Lower Cambrian, Valdai Complex, Lyaminarite Suite. K–Ar, (577 ± 58) 10^6 yr (*23*).

88. Serdobsk Orientation, U.S.S.R. Glauconite from Lower Cambrian sandstone with relicts of pollen (*Paleopirosaceus atavus* Naum, *Paleopirosaceus poroscus* Naum, *Phosphosphaera laminarita* Naum). K–Ar, (610 ± 61) 10^6 yr (*23*).

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The Moon as a Collector of Biological Material

Terrestrial microorganisms may be found on the moon, but extra-solar-system biota would be undetectable.

Edward Anders

Several authors have discussed the possibility that the moon might serve as a repository for microorganisms from outside the solar system, or "astroplankton" (*1–4*). In considering the quantitative aspects of this idea, the first question to be asked is, what mass, W, of microorganisms needs to be ejected from an average planet in our galaxy to give the minimum detectable density of one microorganism per square meter of lunar surface (*1*), or 10^{-12} g/m^2. If this material were distributed uniformly on the moon, the total mass q of such viable microorganisms to be deposited on the moon during its existence would then be $\geqq 38$ grams.

For a single planet at a distance R from the moon, the solid angle subtended by the moon is $\pi r^2/4\pi R^2$, where r is the radius of the moon. If we approximate the galaxy by assuming a planar, uniform distribution of stars (*5*) centered on the moon, the number of stars between the limits R and $R + dR$ equals

$$N(R)dR = 2\pi R\beta dR \qquad (1)$$

where β, the density of stars per unit area, equals 1.4×10^{-25} star per square kilometer, for an effective galactic radius of 50,000 light years and a total star count of 10^{11}.

We shall henceforth make the optimistic assumption that the number of life-bearing planets in the galaxy also equals 10^{11}. The total mass of microorganisms deposited is then given by

$$q = \int W \frac{\pi r^2}{4\pi R^2} 2\pi\beta R dR$$

$$= \frac{\beta\pi W r^2}{2} \ln R + C \qquad (2)$$

Taking this integral between the limits 4.3 light years (the distance of α Centauri) and 50,000 light years, the effective radius of the galaxy, we obtain for W the value 6.0×10^{18} grams. This is the average mass of microorganisms that must be ejected from each of 10^{11} planets to give a density on the moon of one microorganism per square meter.

It is instructive to compare this figure with the mass of the earth's biosphere. The amount of carbon in the biosphere has been estimated (*6*) as 2.8×10^{17} grams, and the total mass of the biosphere may hence be perhaps ten times that amount, or 2.8×10^{18} grams. The mass of a planet's biosphere is dependent on the light flux, the rate of photosynthesis, the metabolic rate of its biota, the climatic conditions, and so on, but in the absence of further knowledge, one may perhaps consider the earth as typical. It then follows that, on the average, no less than twice the mass of a planet's biosphere must be ejected to give a detectable number of microorganisms on the moon's surface. This figure, moreover, makes no allowance for death during ejection, interstellar travel, arrival, and storage on the moon.

Capture and Ejection

We may next consider the processes of capture and ejection. Particles from outside the solar system will arrive on the moon with a velocity equal to the vector sum of the escape velocity from the solar system at 1 astronomical unit (42 km/sec), the earth's circumsolar velocity (30 km/sec), and the lunar escape velocity (2.4 km/sec). In the absence of an atmosphere, even very small particles will vaporize on impact. No information exists concerning the impact dynamics of gram- to kiloton-sized particles at these velocities, but if the data on the Canyon Diablo impact ($\sim 6 \times 10^4$ tons at 15 km/sec) may be used as a guide, more than 99 percent of the material may be expected to vaporize (*7*).

The ejection, whatever its mechanism, requires that the escape velocity from the combined gravitational fields of the planet and the central star be exceeded. The latter term is likely to be large for most life-bearing planets, since the requirement of surface temperatures permitting the existence of liquid water implies close proximity to the central star. Greater distances and lower escape velocities will be found only in the case of very luminous central stars of early spectral classification, since the temperature will vary approximately as the fourth power of the mass and the inverse one-half power of the distance, whereas the gravitational force will vary as the first power of the mass and the inverse second power of the distance. However, these stars remain on the main sequence of the Hertzsprung-Russell diagram only for times of $\leqq 10^8$ years, times which may not be sufficient for the development of life.

It is therefore necessary to consider ejection mechanisms that can propel at least some biological material to the required high velocities. Meteorite and comet impact appears to be the most promising possibility, as pointed out by O'Keefe (*8*), but one must not take it for granted that the sources of these objects, an asteroidal and a cometary belt, are indeed universal properties of all planetary systems. Moreover, the small mass deficit of lunar craters shows that the velocities imparted to the bulk of the material thrown out in such impacts are appreciably smaller than the lunar escape velocity (2.4 km/sec), though the length of Tycho's rays indicates that a moderate amount of material must have nearly reached circumlunar velocity (1.7 km/sec). For any reasonable velocity distribution peaking below 2 km/sec, only a minute fraction of the material is likely to attain the high velocities required to permit its escape from the gravitational field of the planet and its central star. To this factor one may add the effect of the drag of the planet's atmosphere, which will tend to prevent the escape of all but the largest fragments.

It might be argued that ejection in the direction of the planet's motion will permit escape from the system at lower ejection velocities. However, such an

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event requires a head-on collision with an object in a retrograde orbit. Such objects are exceedingly rare in our solar system, and it may not be unreasonable to suppose that they are rare in other systems as well.

It should be noted, incidentally, that this mechanism is operating on the earth not far from its maximum theoretical efficiency. Öpik (*9*) has calculated the frequency of collision with celestial bodies for the earth and finds that an impact resulting in a lethal area equal to the entire surface of the earth is likely to occur every 2.4×10^{10} years. Clearly, a substantially higher collision frequency would lead to the periodic obliteration of all life from a planet. If the intervals between these cataclysms become shorter than the time required for the reappearance of life (reappearance either by chance, divine force, or panspermia), the planet will remain permanently lifeless.

If this ejection mechanism is operating on the earth at close to peak efficiency, then the moon has been periodically contaminated with terrestrial life throughout geologic time. The solid angle subtended by the moon, as seen from the earth, is 5.12×10^{-6}. If the earth has ejected organic matter at the same rate as we assumed for 10^{11} other planets in the galaxy, one finds from the figures cited above that the ratio of terrestrial to extra-solar-system matter on the moon should be $\geqq 8 \times 10^{11}$. This ratio would be even greater if allowance were made for the lower escape and infall velocity of terrestrial matter.

Conclusions

As shown above, the absolute number of extra-solar-system microorganisms is likely to be below the threshold for detection by many orders of magnitude. Now it is clear, in addition, that these few strangers are vastly outnumbered by their terrestrial counterparts. This would also be true, though to a lesser degree, of any Martian microorganisms, which should be outnumbered by a factor of 3.3×10^6. Hence, the odds against a search for extra-solar-system biota being successful are overwhelming, but, as pointed out by Turkevich (*10*), the moon may well yield samples of terrestrial microorganisms now extinct on the earth. At the observed frequencies of meteorite infall (*11*), crater-forming impacts of the required magnitude may be expected at 10^5- to 10^6-year intervals, and a fraction of the debris ejected from the earth may ultimately reach the moon, after transit times ranging from a few days to $\sim 10^7$ years. Indeed, the latest addition to the moon may have taken place in this century, after the fall of the Tunguska meteorite (or comet) in 1908.

It would seem that these facts warrant a reconsideration of the goals of the lunar biology program. Aside from the possibilities discussed by Sagan (*1*), the principal remaining objective would be a test of the panspermia hypothesis, as provided by the presence or absence of terrestrial microorganisms (*4*). If a positive result could be predicted with certainty on the basis of present knowledge, the sterilization of lunar probes would no longer seem to be as necessary as it has sometimes been supposed to be (*2*), although a strong case could still be made for the sterilization of planetary probes.

In conclusion, it should be said that the foregoing calculations do not refute Arrhenius's panspermia hypothesis. It is true that the deposition of 3.8×10^{13} extra-solar-system microorganisms has been shown to be virtually impossible. But Arrhenius's hypothesis requires that only a *single* such spore arrive at the earth. The odds for such an event are quite favorable, and it may have occurred more than once in the earth's history. We may all be descendants of this spore (*12*).

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Science in the News

Disarmament Review: It Will Clarify Some Touchy General Points, But Working Out the Details Is Difficult

The Administration's review of American disarmament policy will not be completed until mid-summer, but the general outline of at least that portion that will be made public is foreshadowed both by the tone of official and unofficial statements by the Administration and by the nature of the problem. Part of this likely course, in fact, was suggested as well as anywhere, in the recent special message on the defense budget.

"Meaningful defense budget decisions," the message said, "are not possible without preliminary decisions on defense policy, reflecting both current strategic assumptions and certain fundamental principles." It then offered a section of eight points, under the general heading "Basic Defense Policies." Each point consisted of a general statement ("Our arms will never be used to strike the first blow." "Our arms must be subject to ultimate civilian control and command at all times." "Our defense posture must be designed to reduce the danger of irrational or unpremeditated general war.") followed by a more detailed explanation of what the policy means.

Point 1 was "The primary purpose of our arms is peace, not war," and the explanatory statements read, in part: "The basic problems facing the world are not susceptible to a military solution. Neither our strategy nor our psychology as a nation—and certainly not our economy—must become dependent upon the permanent maintenance of a large military establishment. . . . Disarmament, so difficult, and so urgent, has been much discussed since 1945, but progress has not been made. Recrimination in such matters is seldom useful, and we for our part are determined to try again. In so doing, we note that, in the public position of both sides in recent years, the determination to be strong has been coupled with announced willingness to negotiate. For our part, we know there can be dialectical truth in such a position, and we shall do all we can to prove it in action. This budget is wholly consistent with our earnest desire for serious conversation with the other side on disarmament."

The effect of the explicitness and clarity that marked the message was to give a sense of coherence and direction that made credible the claim that the increased defense budget was not inconsistent with an increased interest in disarmament.

Comment among observers at the U.N. tended to be highly favorable, for example, even though the observers were primarily interested in disarmament and the message, in its particulars, was devoted to increasing armaments.

Something similar showed up in the White Paper on Cuba last week: although it was a propaganda document, the statement did not confine itself entirely to making a case against Castro. Although it did not dwell on such points, it at least mentioned that past United States policy toward Cuba had not been flawless and that the Castro Government was not an entirely unmitigated evil. It stated flatly that no Cuban government could expect to turn back the clock on the genuine reforms Castro had put through.

The disarmament review can be expected to produce, among other things, a similarly coherent and explicit statement of American thinking in the area of disarmament. Like the defense message, the Cuban White Paper, or the Administration's public papers generally, it is not likely to contain anything greatly surprising to people familiar with the views expressed by the President and his associates in recent years. But the mere restatement of these views in a clearly written official policy declaration will serve two important purposes: to bring these views to the attention of that large part of the world which does not follow closely the details of American politics, and to assure those who do that what the President and his associates have been saying unofficially is in fact the official policy of the government.

Officials see at least three sore points that need elucidation: our attitude toward the Russian slogan "complete and general disarmament"; our attitude toward what has become, but may not remain, our slogan "arms control"; and our attitude on the central problem of inspection. The concern with the first two is primarily a concern with propaganda effects. What is at issue is not the substance of the scheduled negotiations but the effect on world opinion of our use of these terms, both of which have come to carry a good deal of emotional impact. Our attitude toward inspection will be the basis of our position at the bargaining table, although our explanation of the basis of our policy will have effects in the sphere of propaganda.

General Disarmament

Briefly summarized, our attitude on these three points, as suggested by recent statements of the President and his associates is this:

Complete and general disarmament: This has been the Soviet slogan since the time of Litvinov and the disarmament negotiations of the early 1930's. At one time we went along with a U.N. resolution endorsing this as the proper goal of disarmament negotiations, but we have always openly regarded the use of the term as a propaganda stunt by the Russians, which laid us open to the question of why, if we so regarded the term, and if we were really serious about disarmament, we voted for a U.N. resolution which explicitly approved the term.

No government, not even the Russians', seriously maintains that complete and general disarmament is a realistic short-range goal, but nearly all see it as a final goal, something to be aimed at. A frequent type of comment encountered at the U.N. is to have someone concede that the term is often used by the Russians for propaganda effects, but to insist that "even if the Russians aren't serious about it, we are. We don't like to see America sneer at it."

All of this has had two bad effects. By both occasionally endorsing the idea but spending most of our time poohpoohing it we have given an impression

that we don't quite know what we want. We have also given the impression, by this same ambiguous attitude toward this Soviet slogan, that we fear the Russians are so much cleverer than we that they will surely swindle us out of our eye teeth if we so much as blink our eyes. Neither impression contributes to the effect a great nation would like to produce before the rest of the world.

The most likely way to deal with this is fairly obvious, and was tried, for example, by former Secretary Herter in a speech over a year ago: this is to point out that complete disarmament implies world law and a world police force capable of enforcing that law as a substitute for the individual military power which nations must now rely on as their last resort in settling disputes.

Two months ago John J. McCloy, Kennedy's chief of the Disarmament Administration, reiterated this point: "What we . . . must understand," he said, speaking to an American audience, "is that what the world seeks is a peaceful society in which agreed and reliable procedures are set up for the just settlement of disputes, with the concomitant of general and complete disarmament which such a condition permits."

If followed through, as it appears likely to be, an official elaboration of this theme as the policy of the American government, it is hoped, would largely eliminate what has been a continuing American awkwardness about the use of this term, and might well turn it to American advantage, for the Russians have been clearly less interested in enforceable world law than we have, partly on the unstated but clearly implied grounds that such a system might interfere with peaceful coexistence, which Khrushchev has defined as an "intense struggle," short of general war, to communize the world.

Arms Control

The term *arms control* presents a related problem: technically, the term is harmless enough: given the difficulty of achieving actual disarmament argeements and the fact that disarmament, in any case, is subordinate to the broader aim of trying to diminish the likelihood of war, a broader term to cover all the various things, aside from maximizing our deterrent power, that might be done to lessen the chance of war is useful. But the term has, to a large extent, taken on a connotation that implies it is limited to control over armaments only: that is, it is often taken to exclude disarmament. Our use of this term in U.N. discussions has helped the Russians put across their view that we are not interested in disarmament, only in inspection of "arms control," with the consequent opportunity to do a little spying on the side.

We have, in fact, helped give the term this connotation, by the relative passion with which we have talked about inspection, in comparison to the relatively little we have talked about disarmament.

Since the rest of the world is very much interested in disarmament, and not prone to see it pushed aside, the State Department has gradually become aware that our talk of "arms control" is a burden in international discussions. This was an important reason why the agency set up last fall to deal with the question was called the Disarmament Agency rather than the Arms Control Agency.

One of the tasks of the disarmament policy review is to find ways simultaneously to emphasize the importance of things which may not be disarmament in the narrow sense but are nevertheless very important in lessening the chance of war, and to rid ourselves of the burden of the "arms control" slogan, which, useful though the concept is, has taken on connotations that add to the difficulty of America's impressing the world with its case.

Inspection

Our difficulties with both slogans, "arms control" and "general and complete disarmament," stem from our legitimate concern over adequate inspection. We are attracted to the term "arms control" not only because it is a broader term and because it contains areas where progress seems more attainable than in disarmament itself, but because the term itself clearly implies adequate inspection. We have shied away from the term "disarmament" because it does not necessarily carry the connotation of adequate inspection.

The policy review can be expected to come up with an explicit statement of why we must be so concerned with inspection, with a recognition that the Russians have legitimate concerns about inspection, and with an outline of how we think the gap between the contradictory Soviet and American interests can be bridged by balanced concessions on both sides.

Jerome Wiesner, the President's science adviser, and an adviser on disarmament, along with other members of the American delegation to the Pugwash conference last December, offered an informal appraisal of the problem on a recent telecast. They said the Russian and American scientists agreed that our concern for our security and the Russians' concern for their secrecy require that a compromise acceptable to the interests of both parties be worked out in terms of inspection commensurate with the degree of disarmament.

They described some ideas for working out a combination of stationary inspectors, who could cause the Russians no concern about espionage, with roving inspectors, who could cause such concern, arranged so that the Russians would not be subject to a great deal of roving inspection until substantial disarmament had been achieved, while the combination of inspections would offer us assurance that, if the Russians were inclined to cheat, they would be discovered before an unstable imbalance of striking force had developed.

All of this, of course, is easier said than done, and, as the test-ban talks have shown, it is easier to get the Soviet scientists to agree to what is technically correct than to get their political leaders to accept a settlement based on these technical agreements.

Nevertheless, our officials assume that a clearly stated approach to the problem will appeal to the good sense of the rest of the world, so diminishing and perhaps eliminating the unsatisfactory impression our previous statements on disarmament have produced, and they hope this approach will appeal to the good sense of the Russians and lead to some real progress.

But though clear statements of policy have good effects in themselves, these effects do not last long unless a nation shows that it means to act as well as talk.

Prior to his election, Kennedy frequently complained that our negotiating teams have been "ill-staffed, ill-prepared, and ill-advised." Speaking of the Geneva surprise attack conference, Kennedy said we "offered measures which were hastily put together, some of which, even if accepted, were of doubtful value; and others which in reality we were not prepared to accept, or even explain, ourselves." Similar complaints, by friendly critics, have been made of most other disarmament proposals we have offered.

"For 20 years," said Paul Doty, a member of the President's Science Advisory Committee and an attendant

with Wiesner at the Pugwash conference, "the major countries of the world have been putting their labor and treasury into the most complicated and highly organized effort man has ever carried out, and have erected in juxtaposed position an enormous amount of power. One cannot disassemble this casually, because if a mistake is made you risk the very catastrophe that you seek to avoid." The other side of the problem was suggested by Wiesner: "I don't think," he said, "there is time enough to tinker around with small confidence-building measures and leave more comprehensive and more or less total disarmament down to whatever minor deterrent forces you want to leave to preserve stability to some later stage. Science and technology is moving too fast."

Thus it is easy to predict with some confidence the outlines of the policy paper on disarmament that will result from the review, but despite the great urgency, it is hard to predict with any confidence that a detailed plan acceptable to the Russians can be worked out. The situation, on an awesome scale, is the familiar one that it is a great deal easier to create a mess than to clean it up.

News Notes

News Briefs

Radiological health. Steps to reduce the national shortage of specialists in the field of radiological health and of technicians to serve in radiation protection and control programs are discussed in *University Curricula in Radiological Health*, a recent publication of the Division of Radiological Health, U.S. Public Health Service, Washington 25, D.C.

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Water pollution survey. The U.S. Public Health Service has announced results of the most comprehensive survey ever made of the problem of municipal water pollution control. The survey, conducted by the Conference of State Sanitary Engineers, disclosed that the United States needs approximately 5200 new sewage treatment plants and plant enlargements and additions, costing $2 billion.

These projects are required to treat municipal wastes, now being discharged into inland waters, from a population of about 42 million. The Public Health Service estimates that meeting these needs, together with new ones resulting from plant obsolescence and population growth, would cost about $600 million annually. This would be a 40-percent increase in national construction costs for sewage-treatment facilities.

* * *

Chichén Itzá. Divers from Mexico and the United States are retrieving valuable artifacts from the depths of a *cenote* in the ruins of the Mayan city of Chichén Itzá, in Mexico's Yucatán. The sacred *cenote*, or Well of Sacrifice, has yielded hundreds of jade, gold, and copper ornaments, blackened fragments of rare Mayan fabrics, and a few human bones. Exploration of the huge natural well is under the direction of Mexico's National Institute of Anthropology and History in collaboration with the National Geographic Society and the Exploration and Aquatic Sports Club of Mexico.

* * *

International study. The *Handbook on International Study*, a comprehensive guide listing international scholarships, has been published in two separate volumes for the first time, by the Institute of International Education. The two volumes, *Handbook on International Study: For Foreign Nationals* and *Handbook on International Study: For U.S. Nationals*, are published as sources of information for college and university personnel, student advisers, libraries, and others desiring data on international scholarship programs. The volumes can be purchased, for $3 apiece or for $5 a set, from the institute's headquarters (1 E. 67th St., New York).

* * *

Reptile fossil. The 7-inch fossilized skeleton of a gliding reptile older than any previously known to science was found recently in Triassic rocks in an abandoned quarry in New Jersey. The American Museum of Natural History announced the discovery. The reptile lived some 175 million years ago.

New Journals

Agricultural and Biological Chemistry, vol. 25, No. 1, Jan. 1961. T. Mori, president. Agricultural Chemical Society of Japan, c/o Faculty of Agriculture, University of Tokyo, Bunkyo-ku, Tokyo, Japan. Monthly. $9 per year.

Estudos Agronómicos, vol. 1, No. 1, Jan.-Mar. 1960. H. Lains e Silva, director. Missão de Estudos Agronómicos do Ultramar, Rua Rodrigo da Fonseca, 103, Lisbon 1, Portugal. Quarterly.

Medical Electronics News, Mar. 1961. R. Rimbach, publisher. Instruments Publishing Company, Inc., 845 Ridge Ave., Pittsburgh 12, Pa. $6 per year.

Science Review, vol. 1, No. 5, Oct. 1960. C. del Rosario, director. National Science Development Board, National Institute of Science and Technology and the Philippine Atomic Energy Commission, P.O. Box 3596, Manila. Monthly. Free of charge.

Journal of the National Research Council of Thailand, vol. 1, No. 1, Nov. 1960. B. Kalakicha, Ed. National Research Council, Phya Thai, Rama VI Road, Bangkok, Thailand. Quarterly. $5 per year.

Abstracts of Human Developmental Biology, vol. 1, No. 1, Jan. 1961. G. ten Cate, Ed. Excerpta Medica Foundation, 119-123 Herengracht, Amsterdam, The Netherlands. Monthly. $17 per volume.

Los Alamos Scientific Laboratory Quarterly Review, Winter 1961. Los Alamos Scientific Laboratory, University of California, Los Alamos, N.M.

Chesapeake Science, vol. 1, No. 2, June 1960. R. J. Mansueti, Ed. State of Maryland, Department of Research and Education, Chesapeake Biological Laboratory, Solomons, Md. Irregular. $2 per year.

Applied Optics, vol. 1, No. 1, Jan. 1962. J. N. Howard, Ed. Optical Society of America, 1155 16th St., NW, Washington 6, D.C. Bimonthly. Members, $6 per year; nonmembers, $10.

Materials Research & Standards, vol. 1, No. 1, Jan. 1961. R. E. Hess, Ed. American Society for Testing Materials, 20th and Northampton Sts., Easton, Pa. Monthly. $5 per year.

Bulletin of the Hiroshima Agricultural College, vol. 1, No. 3, 1960. Hiroshima Agricultural College, Saijo, Hiroshima Prefecture, Japan.

Problems of the North (complete translation of the Russian journal *Problemy Severa*), No. 1, Dec. 1960. M. Dunbar, Ed. Translations Section, The Library, National Research Council of Canada, Sussex Drive, Ottawa 2, Canada. $7 per issue; single papers, $1.

Kybernetik, vol. 1, No. 1, Jan. 1961. Springer-Verlag, Heidelberger Platz 3, Berlin-Wilmersdorf, Germany. Irregular. Maximum price for 1961, DM. 80.

Scientists in the News

T. Dale Stewart has been appointed head curator of the department of anthropology, U.S. National Museum, Smithsonian Institution, filling the position recently left vacant by the retirement of Frank M. Setzler. Stewart has been a member of the staff of the museum since 1927; he has been curator of the division of physical anthropology since 1942.

Loren Eiseley, author, anthropologist, and provost of the University of Pennsylvania, has been awarded the 1961 John Burroughs medal in recognition of his most recent book, *The Firmament of Time*. The John Burroughs Association presents the award annually for literature which combines excellence and accuracy and which is based on original observation and conclusion.

Philip E. Ryan, executive director of the National Health Council since 1953, has been named executive director of the National Association for Mental Health, effective 1 May.

Kenneth T. Bainbridge, Harvard University physicist known for his research on isotopes and radioactivity and for his work on techniques for weighing atoms, has been named George Yasmer Leverett professor at Harvard. The professorship was established in 1958 through a bequest made by Leverett, a lawyer for Bell Telephone Companies and a graduate of Harvard and of Harvard Law School.

Kenneth T. Bainbridge

C. H. Waddington, professor of animal genetics at the University of Edinburgh, Scotland, will deliver the Jesup lectures at Columbia University. The lectures will be given on 18, 20, 25, and 27 April and on 2 and 4 May. Waddington's subject will be new patterns in morphogenesis.

D. W. Pearce, manager of chemical effluents technology for General Electric's Hanford Laboratories, Richland, Wash., has been appointed director of health, safety and waste disposal for the International Atomic Energy Agency in Vienna, Austria. He will begin a 2-year assignment on 15 April.

J. A. Van den Akker, senior research associate and chairman of the department of physics and mathematics at the Institute of Paper Chemistry, Appleton, Wis., has been invited by the University of Manchester, England, to be a lecturer in physics during the 1961–62 academic year. He has been granted a Fulbright award.

Joseph P. Greenberg, former head of the section on chemotherapy, Laboratory of Malaria Studies, National Institute of Allergy and Infectious Diseases, Bethesda, Md., and former program director in microbiology, Stanford Research Institute, Menlo Park, Calif., has joined the staff of the Palo Alto (Calif.) Medical Research Foundation as chief of the new Microbiology Division.

Several visiting scientists from Australia have recently arrived in the United States.

M. G. Baillie, of the chemical engineering section of the Australian Atomic Energy Commission's Research Establishment, Lucas Heights, New South Wales, has arrived for a 2-year stay at Oak Ridge National Laboratory, where he will work in the Chemical Technical Division on the engineering evaluation and development of sulfate extraction contactors.

E. W. B. Da Costa, of the Commonwealth Scientific and Industrial Research Organization's Division of Forest Products, South Melbourne, will visit scientific institutions for 2 months, studying techniques for laboratory and field evaluation of the durability of wood, wood preservatives, and the physiology and taxonomy of wood-destroying fungi. His itinerary includes the University of California; the Forest Products Laboratory, Vancouver, B.C.; the Forest Products Laboratory, Madison, Wis.; the Southern Forest Experiment Station, New Orleans; Koppers Ltd., Pittsburgh; the New York State College of Forestry, Syracuse; the University of Maryland; the U.S. Department of Agriculture's Forest Disease Laboratory, Beltsville, Md.; Yale University; the Connecticut Agricultural Experiment Station; and the Department of Agriculture of Canada, in Ottawa. He will leave for the United Kingdom on 14 June.

E. S. Pilkington of the Commonwealth Scientific and Industrial Research Organization, Division of Mineral Chemistry, Melbourne, will be in North America for about 2 months, visiting centers of research in nonferrous metallurgy. He will visit the University of California, Berkeley; the U.S. Geological Survey, Denver; the University of Minnesota; Argonne National Laboratory; Wayne State University; the U.S. Bureau of Mines and the Westinghouse Research Laboratories, Pittsburgh; Oak Ridge National Laboratory; the U.S. Geological Survey, Washington, D.C. (10–12 May); and the University of Toronto and other scientific institutions in Canada. He will leave for the United Kingdom on 28 May.

Kenneth M. Brinkhous, chairman of the department of pathology at the University of North Carolina School of Medicine, was recently named recipient of the O. Max Gardner award. The award is made to the member of one of the faculties of the Consolidated University of North Carolina who, during the current scholastic year, "has made the greatest contribution to the welfare of the human race." Brinkhous is known for his research on hemophilia.

Leland G. Merrill, Jr., Rutgers University entomologist, has been named dean of the university's College of Agriculture and director of the Agricultural Experiment Station. He succeeds William H. Martin, emeritus dean of the College of Agriculture, who retired last June.

Joseph T. Velardo, assistant professor of anatomy at the Yale University School of Medicine, has been appointed professor and chairman of the department of anatomy of New York Medical College, effective 1 July. He succeeds J. Clifford Hayner, who will become professor emeritus in residence at the college.

S. S. Barkulis, director of microbiological research with CIBA Pharmaceutical Products, Inc., Summit, N.J., has been awarded the eighth annual Selman A. Waksman award by the New Jersey Branch of the Society of American Bacteriologists. The award is presented every year to a scientist in the eastern section of the United States who is under 40 years of age.

Walter P. Work, associate clinical professor of otolaryngology at the University of California School of Medicine, has been named chairman of the department of otolaryngology at the University of Michigan Medical School, effective 1 July. He succeeds the late James H. Maxwell.

Alexander Kossiakoff, pioneer in the field of solid propellant rockets and a well-known physical chemist, has been named associate director of Johns Hopkins University's Applied Physics Laboratory in Silver Spring, Md. He has been assistant director of the Laboratory since 1948.

Harold L. Segal, associate professor of pharmacology at the St. Louis University School of Medicine, has been awarded a 6-month National Science Foundation fellowship to conduct research at the University of Vienna in Austria, where he will investigate the chemical structure of proteins. He will be on leave of absence until 1 September.

Ralph A. Sawyer, vice president for research and dean of the graduate school at the University of Michigan, recently received the Spectroscopy Society of Pittsburgh's annual award for outstanding contributions to the field of spectroscopy. The presentation took place during the Pittsburgh Conference for Analytical Chemistry and Applied Spectroscopy, which was attended by more than 3500 scientists.

Col. **Charles H. Roadman** of the National Aeronautics and Space Administration, since last June a special assistant to the director of the Office of Life Sciences, has been appointed deputy director of that office. Roadman, a career Air Force officer, is a specialist in aviation medicine.

George Welford has joined U.S. Nuclear Corporation, Burbank, Calif., as director of its Laboratory for Radiochemical Bioassay and Environmental Testing. Welford was formerly with the Atomic Energy Commission's Health and Safety Laboratory, where he was leader of the Radiochemical Methods Development Group.

George C. Sponsler recently joined the Research and Development Division at the Navy Bureau of Ships as chief scientist for research and development on the Technical and Analysis and Operations Research Staff. Previously, Sponsler was senior scientist at the Hoffman Science Center, Santa Barbara, Calif.

Benjamin Pasamanick, professor of psychiatry at Ohio State University and director of research at the Columbus Psychiatric Institute, has received the $500 Stratton award of the American Psychopathological Association for his studies on the epidemiology of mental disorder.

James W. Perry recently became a member of the faculty of the systems engineering department at the University of Arizona's College of Engineering. He was previously director of the Documentation Center at Western Reserve University. Application of electronic automation to non-numerical information is his principal field of interest.

I. Moyer Hunsberger has been named dean of the College of Arts and Sciences at the University of Massachusetts. A former Fordham University professor, he joined the university last September to head its department of chemistry. In his new post he succeeds **Fred V. Cahill, Jr.**, who resigned last July to become dean of general studies at North Carolina State College.

After 38 years of service with the U.S. Department of Agriculture, **John T. Scanlan** has retired as head of lubricants investigations at the Agricultural Research Service's Eastern Utilization Research and Development Division in Wyndmoore, Pa. Scanlan is best known for his research program on animal fat, which led to the development of some important commercial products, including a purified oleic acid and a stabilizing plasticizer that enables plastic compositions to withstand the effects of heat and light. He also developed several processes for obtaining lanolin derivatives from wool wax.

Recent Deaths

Parry Borgstrom, Washington, D.C.; 71; superintendent of the U.S. Naval Research Laboratory's Chemistry Division from 1933 until his retirement in 1954; received the Navy's Distinguished Civilian Service Award in 1946; was instrumental in the development of anti-chemical-warfare equipment, aids for personnel downed at sea, and improved methods of fighting oil fires; 25 Feb.

David M. Gould, Denver, Colo.; 48; chairman of the department of radiology at the University of Colorado School of Medicine; formerly department head at the University of Arkansas Medical Center; 1 Apr.

Harry E. Hammond, Columbia, Mo.; 77; emeritus professor of physics at the University of Missouri, where he taught for 35 years; retired in 1955; 18 Mar.

Thomas H. Lanman, Chestnut Hill, Mass.; 69; clinical professor of surgery, emeritus, at Harvard University, where he served from 1928 to 1957; was a pioneer in the surgical treatment of pulmonary disorders in infants and young children; 25 Mar.

George F. Myers, Jackson Heights, N.Y.; 96; pioneer in aviation and a contemporary of the Wright brothers; credited with inventions pertaining to the earliest heavier-than-air flying machines, helicopters, and parachutes; was a practicing patent attorney; 5 Apr.

Howard C. Naffziger, San Francisco, Calif.; 76; pioneer brain surgeon and former president of the American College of Surgeons; retired in 1952 from the University of California as emeritus professor of neurological surgery and a regent of the university; was a department chairman for many years; developed diagnostic tests for tumors of the central nervous system and attained international renown for surgical techniques he devised for removal of tumors of the brain and pituitary gland; 22 Mar.

Harry Raymond, Albany, N.Y.; 85; astronomer; retired in 1939 from the Dudley Observatory, Albany; 23 Mar.

John Unrau, Edmonton, Alberta; 45; head of the department of plant science at the University of Alberta and past president of the Genetics Society of Canada; contributed to research in cereal genetics; 1 Mar.

Erratum: In the editorial for 7 April, "Equal but separate," the third sentence of the last paragraph should have read: "In the alternatives of no federal aid or federal aid with some of it going to racially segregated schools, the choice must be for the second alternative."

Book Reviews

T. H. Huxley, Scientist, Humanist and Educator. Cyril Bibby. Forewords by Sir Julian Huxley and by Aldous Huxley. Horizon Press, New York, 1960. xxii + 330 pp. Illus. $5.

Rarely does one find such a book as this. The outward signs proclaim a standard biography, but the volume is far from that, even though supplied with such scholarly appurtenances as biographical footnotes on every public figure named and impressive bibliographical citations of the original works, minutes, and letters pertaining to Thomas Henry Huxley. The treatment is definitely nonchronological, although to compensate for this the author has included a highly interesting tabular array of information at the end of the volume, in the form of a Conspectus of T. H. Huxley's Life and Times. Here, year by year throughout Huxley's life, one can note, in eight parallel columns, the chief events in British government and social, cultural, and educational background, as well as personal information, significant publications, selected lectures, and scientific society happenings.

What marks the book as distinctive —and indeed distinguished—is its point of view. Instead of attempting to look at the phenomenon of Thomas Henry Huxley in full, Cyril Bibby, himself a teacher and a teacher of teachers, has chosen to concentrate attention on Huxley's intense devotion to matters of education and his primacy in extending education in Britain to every child and in forcing upon a stagnant system of higher education some appreciation that the natural sciences must form a part of the liberal education due modern citizens. Everything else is peripheral and secondary in interest to this emphasis. Huxley the scientist, Huxley the humanist, and Huxley the family man yield place to Huxley the educator.

Cyril Bibby's style is readable and often graphic, and the portrait that results is as finely drawn as the lineaments of Huxley's changing features, which grace this book in the numerous photographs and drawings that represent him, seriously or in caricature, at various ages. Only two items are drawbacks: one, that the deliberate disregard of chronology makes one shift gears, with considerable grinding, at frequent intervals; the other, that in some chapters there is a plethora of British school jargon, such as "Responsions," "Moderations," "Tripos," "the Grace," and the like which may require some explanation for American and other foreign readers. These are quite minor matters compared to the merits of the book.

Huxley, the Educator

The first four chapters constitute a broad introduction to the educational theme. With "Science for the Citizen" we are fairly launched into an account of Huxley's absorption in active teaching and his promotion of better educational programs. Throughout Huxley's stout defense of Darwinism and his clash with the clergy, throughout his scientific labors and his political sallies, and throughout his concern with morality and metaphysics, these matters clearly remained the dominating activity to which his energies were directed. How he built the first School of Science in England, how almost singlehandedly he reformed the miserable state of secondary school education while serving on the London School Board, his influence on the British public schools, on the older English universities and on those of Scotland, culminating in his final stupendous struggle—when old and sick—to draw together the divided and substandard colleges of his city into the University of London—these narratives make a stirring tale for anyone convinced that education is a major problem of society.

Huxley's Concepts: Then and Now

To Thomas Henry Huxley we owe certain well-accepted ideas—the conviction that the ordinary workingman is often competent and eager to learn, and the emphasis on the laboratory as the basis of the best teaching in the sciences. But we also find him voicing revolutionary conceptions that even now need reemphasis. These include a redefinition of liberal education to include the sciences as a major, integral part of the education of every citizen living in an age indubitably transformed by scientific developments; the importance of the arts along with the sciences; the need for morality, integrity, and doubt to accompany will and intellect; and the right of every child to have educational opportunity in proportion to his capacity and willingness to learn. Those in government today who are considering broad educational programs in the sciences would do well to read Huxley on these subjects, for nearly a century ago he had grappled with most of these problems and reached sound conclusions.

Huxley's career was marked, as Bibby says, by extraordinary effectiveness. This was in part because he was a veritable "agnostic tornado of energy," able to work incessantly and at a dozen tasks at one time. It was also because he knew men—knew when to be stubborn and unrelenting and just when to concede a minor point in order to gain a major concession. And, of course, his effectiveness owed much to his great mastery of expression. Clarity and lucidity marked his prose, both spoken and written, and were matched by forcefulness. Although he respected sincerity, he tolerated no shoddy logic in himself or in others. The result was a hard-hitting eloquence that was as feared by Gladstone and Newman as by Bishop Wilberforce, by Richard Owen as by the Duke of Argyll. The temptation to quote some of his sayings about education is quite irresistible.

> Our sole chance of succeeding in a competition, which must constantly become more and more severe, is that our people will not only have the knowledge and the skill which are required, but that they shall have the will and the energy and the honesty, without which neither knowledge nor skill can be of any permanent avail.
>
> There are a great many people who imagine that elementary teaching might be properly carried out by teachers provided with only elementary knowledge. Let me assure you that this is the profoundest mistake in the world. There is nothing so difficult to do as to write a good elementary book, and there is nobody so hard to teach properly and well as people who know nothing about a subject.
>
> . . . nothing can now prevent it [scientific knowledge] from continuing to distil up-

wards and permeate English society, until, in the remote future, there shall be no member of the legislature who does not know as much of science as an elementary schoolboy.

. . . the great aim should be to teach only so much science as can be taught thoroughly; and to ground in principles and methods rather than to attempt to cover a large surface of details.

. . . in the actual condition of the nation . . . I cannot doubt the wisdom of enforcing the teaching of science upon the public schools by positive enactment.

If individuality has no play, society does not advance; if individuality breaks out of all bounds, society perishes.

No personal habit more surely degrades the conscience and the intellect than blind and unhesitating obedience to unlimited authority. Undoubtedly, harlotry and intemperance are sore evils, and starvation is hard to bear, or even to know of; but the prostitution of the mind, the soddening of the conscience, the dwarfing of manhood are worse calamities.

Let the reader decide which of these sayings are relevant to our own times and our present national condition.

Huxley's New World

The last chapter of the book is entitled "To the New World," but it includes more than a discussion of Huxley's trip to the United States and his address upon the occasion of the founding of the Johns Hopkins University. The chapter concludes with a broader consideration of the really "new world" which Huxley always had in mind as the fruition of his hopes and the goal of his efforts, a "new world" in which the scientific method would be supreme.

"We are in the case of Tarpeia," he declared on one occasion, "who opened the gates of the Roman citadel to the Sabines and was crushed by the weight of the reward bestowed upon her. It has become impossible for any man to keep pace with the progress of the whole of any important branch of science. It looks as if the scientific, like other revolutions, meant to devour its own children; as if the growth of science tended to overwhelm its votaries; as if the man of science of the future were condemned to diminish into a narrow specialist as time goes on."

Only by sharing the rewards might we escape Tarpeia's fate, only by so organizing and extending scientific education that breadth of culture might be secured without superficiality, depth and precision of knowledge without narrowness. To achieve this remains today, as in Huxley's time, the major task of education. What Huxley foresaw so clearly a century ago, we may all see clearly today as the imminent threat to further progress. Before addressing ourselves to this task, we might well reread Huxley on education, or, in lieu of that, peruse this farsighted, spirited book which for the first time treats adequately Huxley's dedication to science in education, and to education in science.

BENTLEY GLASS
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Physics of the Upper Atmosphere. John A. Ratcliffe, Ed. Academic Press, New York, 1960. 586 pp. Illus. $14.50.

In the past 10 years our knowledge of the earth's upper atmosphere has gone forward to an extent unparalleled during any previous period. The subject matter has expanded greatly and now includes for the first time, information about the nature of the solar radiation that provides the driving force for so many atmospheric phenomena. The names of the topics are very much the same as they are in the classic treatise by Mitra, but the content is remarkably different. This is evidenced at once by the introduction by Sydney Chapman, a man who has contributed so outstandingly to every facet of the physics of the upper atmosphere, not to mention his many other contributions to pure physics. Here he views the problem of the interaction between the solar corona and the earth's outer atmosphere (the exosphere, and its ionized portion, the protosphere).

M. Nicolet of Belgium addresses himself to the problem of the distribution of atmospheric constituents in the ionospheric region. He gives an excellent general review of the whole atmosphere but then concentrates on such topics as the dissociation of molecular oxygen and the problem of diffusion. There is a particularly detailed discussion of the problem of heat flow in the upper atmosphere. Finally, satellite data are used to give information on the structure of the atmosphere up to about 700 kilometers.

The next chapter, by Homer E. Newell, Jr., is an authoritative review of data on the upper atmosphere obtained by rockets and satellites. The emphasis is on rockets, and most of the information deals with atmospheric structure, winds, and density of ionization. It is extremely useful to have this review which also incorporates data obtained during the International Geophysical Year.

In contrast to the wide scope of the previous chapter, Herbert Friedman's discussion of the sun's ionizing radiations is a detailed account of the state of our present knowledge of x-ray and ultraviolet radiation from the sun and of its interaction with the atmosphere. Most of the important research findings in this area have been made by Friedman and his colleagues at the Naval Research Laboratory.

The next three chapters, dealing with airglow and aurora, are written by David R. Bates of Belfast. The discussion is brief but quite complete. The spectrum intensities and excitation processes are discussed for the nightglow, the twilight glow, and some indication is given of the possibility of a day airglow. The chapters on the aurora again are brief but meaty. The auroral forms are classified; their geographic and temporal distribution is described, and their relation to other geophysical phenomena is shown. It is in the discussion of the auroral spectrum, however, that the author contributes important detail and much new material. The aurora is also described from the point of view of radar, with particular emphasis on the motions associated with auroral echoes, by Henry G. Booker.

The longest and most detailed chapter (318 references), by J. A. Ratcliffe and K. Weekes, discusses all phases of ionospheric physics. Only the subheadings need to be given, "Theory of the origin and shape of layers of electrons," "The ionosphere as a dynamo and a motor," "Theory of wave propagation through the ionosphere," "Undisturbed D-region," "Undisturbed E-layer," "Undisturbed F1-layer," "Undisturbed F2-layer," "The collision frequency of electrons," "Horizontal irregularities and movements," and finally "Disturbances and storms in the ionosphere."

Closely related is the chapter on upper atmosphere and geomagnetism by E. H. Vestine. Here the subject matter has not advanced as rapidly in the last few years. Much of our knowledge of the daily solar variation, and the daily lunar variation is as it was before the IGY. Magnetic storms have been better studied, however, and the subject of magnetic pulsations has come to the foreground. The puzzling

feature of the equatorial electrojet is now understood, and is in harmony with the dynamo theory. The brief chapter on the upper atmosphere and meteors, contributed by J. S. Greenhow and A. C. B. Lovell, deals mainly with radar observations of meteor trails.

A final chapter is supposed to give the advances made during the IGY, and provides something of a catch-all to relate recent results. Except for a discussion of the detailed shape of the solar Lyman-alpha line, this chapter is somewhat disappointing. The rapid pace of development is shown by the fact that the exosphere is still described as an isothermal region. More recent work has shown that this concept is not justified and that the distribution of density in the exosphere varies considerably from that of a gas at constant temperature and in thermodynamic equilibrium.

Except for such rather minor shortcomings, the book constitutes a most useful and up-to-date compendium on upper atmosphere physics. It is a must for the specialist, but is written so that the nonspecialist or student can inform himself authoritatively of the state of this important subject.

S. F. SINGER

Department of Physics, University of Maryland

The Policy Machine. The Department of State and American foreign policy. Robert E. Elder. Syracuse University Press, Syracuse, N.Y., 1960. 238 pp. $4.50.

In *The Policy Machine*, Robert Elder examines the structure and procedure by which American foreign policy is made and administered. The "policy machine," he says, "which grinds out American Foreign Policy is one of the most intricate and complex mechanisms of modern democratic government." Not so long ago the Department of State was a relatively modest agency in which sons of "good families," trained at Ivy League colleges and ignorant alike of their own country and the world at large, could carve out fashionable careers in Washington or abroad. Today this same agency is a bureaucratic behemoth among behemoths. With over 6500 employees in Washington and New York, it operates 277 overseas posts, staffed by over 6000 American citizens and nearly 10,000 alien employees. It is a vastly complex communication center which on an average day receives over 1200 telegrams, dispatches, and operation memoranda and sends more than 1500 communications by telegraph and diplomatic pouch. "During the fiscal year of 1958, a quiet one diplomatically, 7,500,000 words a month flowed in and out of the Department's telegraph room. Some 80% of this communication flow is classified for official eyes only."

Nor is foreign policy contained wholly within the Department of State. The International Cooperation Administration, the Export-Import Bank, the Development Loan Fund, and the United States Information Agency are independent or semiautonomous cogs in the vast foreign policy machine of the United States. So, too, are the overseas representatives of the departments of Agriculture and Treasury, Commerce and Labor. At the summit of this structure, the President, as the sole constitutional voice of the United States in its international relations, must somehow shape, coordinate, and direct people, policies, and programs.

In the conduct of foreign relations, as in other areas of his responsibility, the President must make a kind of Hobson's choice between a manageable number of unmanageable agencies, or an unmanageable number of manageable ones. Even the President's "span of control" has outer limits, and in striving to keep on top of the vast policy machine over which he presides, he must rely on, not one, but a hierarchy of coordinating agencies which stand between him and his agents at the level of operations. His communication with our major diplomatic missions, consular offices, and delegations to international organizations carried on through the Secretary of State, is reasonably direct and, in emergency, can be swift and sure. But for the most part the President's knowledge of events in the field is filtered through an elaborate sieve.

The "low men on this totem pole," if I may mix a metaphor, are the 114 country desk-officers in the State Department, who are "the eyes and ears, the brain and voice of America in a troubled world." Standing between these officers and the President is a veritable wilderness of coordinating agencies and officials including assistant secretaries in charge of African, Inter-American, European, Far Eastern, Near Eastern, and South Asian affairs and bureaus of International Organization, Economic Affairs, International Cultural Relations, Intelligence and Research, Public Affairs, Administration, and Consular Affairs. There are also assistant secretaries responsible for policy planning and Congressional relations. At the apex of this structure is, of course, the official on whom the President must mainly depend, the Secretary of State, with his undersecretaries and deputy undersecretaries for political affairs and administration, who, with the directors of the ICA and the Foreign Service, make up the high command of the Department of State.

But since foreign policy is no longer the exclusive prerogative of the State Department, other interdepartmental agencies of coordination have emerged. Most important of these are the National Security Council, the National Security Council Planning Board, the Central Intelligence Agency, the United States Intelligence Board, and an Operations Coordinating Board. Under President Eisenhower the National Security Council became a kind of supercabinet in which major problems of both foreign and domestic policy were discussed. Included in this body, either by law or invitation, are the President, the Vice President, the secretaries of State, Defense, and Treasury, the directors of Defense Mobilization, the Bureau of the Budget, the U.S. Information Agency, and the Central Intelligence Agency, and the chairman of the Joint Chiefs of Staff.

This, in rough outline, is the policy machine which Elder strives to describe and to evaluate. Although Elder knows that our foreign policy is a complex of diplomatic, economic, military, and cultural relations, his major concern in this volume is with its diplomatic and cultural aspects. His book is more than a study of structure and organization, although its main emphasis is on problems of administration and personnel. But in dealing with these problems, Elder takes us behind the scenes to look with a friendly but critical eye at some of the more dynamic aspects of politics and administration as they affect our foreign policy. His analysis of the role of public information officers, public opinion analysts, and legislative liaison specialists in the foreign policy machine is informative and perceptive. All students of public administration will find the author's chapter on personnel management of particular interest, since he does not hesitate to grasp the Wriston nettle with a firm hand.

Whether "the need for specialization can be met without a major retreat from Wristonization," whether the Government should "create its own undergraduate Foreign Service Academy," whether the merit system should be so extended as to exclude political appointments altogether are but some of the ticklish issues which Elder discusses with vigor and insight. Not everyone will agree with the answers he suggests, but no one can quarrel with his statement that "a creative adaptation of organization and policy to a world . . . in flux is essential if America and Western Civilization are to avert a decline similar to that of so many great nations and civilizations of the past."

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The Grasses of Burma, Ceylon, India and Pakistan (Excluding Bambuseae). N. L. Bor. Pergamon, New York, 1960. xviii + 767 pp. Illus. $25.

The Grasses of Burma, Ceylon, India and Pakistan replaces volume 7 of Sir Joseph Hooker's *Flora of British India*. It is a must for anyone interested in agrostology, for it contains a wealth of carefully compiled, concise information on most phases of the subject. Moreover, it is of much importance to the agriculturist interested in forage crops, grazing, or soil conservation: there are many notes on these subjects, as well as lists of species adapted to these purposes. The foreword, written by George Taylor, director of the Royal Botanic Gardens at Kew, includes a brief history of work at Kew on Indian grasses and a paragraph about the author.

The book is divided into a general part and a systematic part. The general part includes a chapter on the morphology of the grass plant which the author divides into the vegetative shoot, the reproductive shoot, and the flower. Numerous examples are given to illustrate the terms. There follows a chapter on dispersal of grass fruits and seeds, by wind, water, and special devices, also with illustrative examples. The third chapter discusses the uses of grasses for food, grazing, and fodder. A list of introduced and exotic fodder grasses is provided, among which are some of our own native grasses. Grasses for essential oils include 15 species of *Cymbopogon*. A brief account of the oil and oil production from each is given. Other genera producing aromatic oils are *Vetiveria, Bothriochloa,* and *Capillipedium*. Grasses used for paper making, lawns, soil binders, and miscellaneous uses are noted briefly. It is of interest that *Pennisetum clandestinum* has been "an astonishing success in hills where rainfall is high, and is one of the most valuable introductions. It provides an excellent fodder, forms a close turf, and wears well." The final chapter on obnoxious grasses mentions poisonous grasses, grasses causing mechanical injury, and weeds. Two of the worst weeds are *Imperata cylindrica* and *Saccharum spontaneum*; both are aggressive.

The systematic part presents a brief history of the "old systematics," which covers the period from pre-Linnaean times down to about 1930. The change from the old concept of large genera such as *Andropogon* and *Panicum* to smaller, more homogeneous ones is given as an example of advancement during this time. "No matter how perfect that system may become from the standpoint of the cytogeneticist, anatomist, physiologist and so on, the task of the taxonomist will still be the correct identification of his plants. That task will not be helped by knowing the chromosome number of the plants before him . . ., but will be based upon easily observable characters from which keys can be constructed. . . . After all, the taxonomist's business was to name plants, and in his classification it must be remembered that the remarkable thing about it was not that so much was wrong, but that so much has been proved right."

In the "new systematics" a taxonomist considers, in addition to gross morphological characters, the following topics: size and basic number of chromosomes, leaf anatomy, first seedling leaf, lodicules, embryo, hilum, root hairs, starch grains, nucleoli, and the nature of the shoot apex. A brief account of the importance of each and what has been accomplished is given. There is a brief discussion of the origin of grasses, and the several phylogenetic arrangements that have been proposed. Of particular interest is the statement attributed to C. E. Hubbard, that "the ancient primary grass consisted of leafy-branched flowering shoots, each of which was many-noded and bore at each node a sheathing leaf. In the axil of each leaf would be found a prophyllum-like scale and between it and the leaf-base a typical monocotyledonous flower."

In the systematic list, the grasses are divided into two groups, the Panicoideae —consisting of the tribes Maydeae, Andropogoneae, and Paniceae—and the Pooideae, consisting of 36 tribes, most of them relatively small. The arrangement is strictly alphabetical throughout, an arrangement which greatly increases the volume's usefulness as a ready reference work. It is also a very commendable plan because of the rapid changes taking place in the concepts of grasses. "It seems obvious that a great deal more information must be gained, before even a tentative scheme with a moderate chance of acceptance can be produced."

There are dichotomous keys to the groups, tribes, genera, and species. A complete citation, important synonyms, geographical distribution, uses, principal exsiccatae, and chromosome number, if known, are given for each species. Numerous pertinent taxonomic discussions are included when they are necessary to explain the reasons for using a given name. There are no species descriptions. These would have been very desirable, but they would have doubled, at least, the size of an already large volume. An appendix contains the Latin descriptions of new tribes, genera, and species, including a rather large number of species of *Agropyron*.

The book is well printed and easy to read. There are 80 full-page illustrations, most of them drawings taken from the *Flora of West Tropical Africa, East African Pasture Plants,* and *The Cultivated Races of Sorghum*.

JASON R. SWALLEN
Department of Botany, U.S. National Museum, Smithsonian Institution

Miscellaneous Publications

(Inquiries concerning these publications should be addressed, not to Science, *but to the publisher or agency sponsoring the publication.)*

Carnegie Institution of Washington. The report of the president, 1959–1960. Carnegie Institution, Washington 5, 1960. 74 pp.

Protein Structure and Function. Report of a symposium held 6–8 June 1960. Biology Department, Brookhaven National Laboratory, Upton, N.Y., 1960 (order from Office of Technical Service, Department of Commerce, Washington 25). 266 pp. Illus. $2.50.

La Recherche Scientifique aux U.S.A. Report on a study made under the auspices of the Ford Foundation. André Molitor. Conseil National de la Politique Scientifique, Brussels, Belgium, 1960. 61 pp.

Reports

Isolation of Blastomyces dermatitidis from Soil

Abstract. Through the intravenous inoculation of soil suspensions in the tail vein of mice, *Blastomyces dermatitidis* was recovered from a Lexington, Kentucky, soil sample. The positive specimen was collected in a tobacco-stripping barn. The shed had sheltered a dog that died of blastomycosis 2 years before the sample was collected.

Although soil has been found to be the natural habitat for many of the fungi pathogenic to man (*1*), all previous attempts to isolate *Blastomyces dermatitidis* from this source have failed (*2*). In the present study, soil specimens collected from the environs of human and canine cases of blastomycosis in or near Lexington, Kentucky, were examined by means of a new technique developed at the Medical College of Georgia. *Blastomyces dermatitidis* was isolated from one of the samples.

This sample was collected during the course of a continuing investigation of the natural habitat of the fungus (*3*). Each sample was taken by pressing the open mouth of a sterile 4-oz screw-cap bottle into the soil. The first 600 samples were examined by a technique which is routinely employed by the Mycology Unit of the Communicable Disease Center for the isolation of pathogenic fungi from soil (*4*) and which involved the intraperitoneal inoculation of mice. In addition, soil suspensions were serially streaked on plates of three different media (*3*). Approximately every tenth specimen (54 in all) was sent to the Medical College of Georgia for re-examination by a different technique that involved the intravenous inoculation of mice.

Prior to inoculation, a heaping teaspoonful of soil was suspended in 50 ml of physiological saline containing 1000 units of penicillin G and 1 mg of streptomycin per milliliter in a large (38 by 200 mm) test tube. After vigorous stirring, the soil was allowed to settle for 1 hour. A 5-ml sample was removed with a pipette from the interface between the supernatant and the sediment. Two-tenths of a milliliter of a slightly turbid saline suspension of *Mycobacterium fortuitum* was added as an adjuvant to each milliliter of sample. After thorough mixing, 0.5 ml of the specimen was inoculated intravenously into the tail vein of each of five adult white Swiss mice.

Three weeks after inoculation the mice were killed, and their lungs and small portions of their livers and spleens were cultured. Both lungs, each lung on a different plate, were cultured on Sabouraud dextrose agar containing 100 units of penicillin, 0.1 mg of streptomycin, and 1 mg of cycloheximide per milliliter. Pieces of liver and spleen from each mouse were cultured on tubes of brain heart infusion blood agar containing 100 units of penicillin and 0.1 mg of streptomycin and on neutral dextrose agar with the same antibiotics plus cycloheximide. Thirty cultures were made for each soil sample. To prevent drying, the plates were sealed with cellophane tape, the tubes with rubber stoppers. The Sabouraud agar plates and tubes of natural dextrose agar were incubated at 25°C, while the brain heart infusion blood agar, without cycloheximide, was incubated at 37°C. The cultures were examined after 30 days for the presence of pathogenic fungi.

Blastomyces dermatitidis was isolated from one of the 54 soil specimens examined at the Medical College of Georgia. Of the five mice inoculated with this soil, two yielded isolates of the fungus. Both Sabouraud agar plate cultures of the lungs from each mouse were positive for *B. dermatitidis*. The liver and spleen cultures were negative. At autopsy, gross lesions were not detected in the lungs.

The strain of *B. dermatitidis* isolated from soil was typical of the species (*5*). Growth on Sabouraud dextrose agar after 30 days at 25°C was similar to that of cultures isolated from human beings and dogs with respect to gross colony characteristics and microscopic morphology. When two subcultures were made on brain heart infusion blood agar and incubated at 37°C, they were converted to the typical yeastlike form in 10 and 15 days, respectively. Subcultures of the yeastlike form on brain heart infusion blood agar incubated at 37°C grew rather slowly and sparsely. When transferred to a 25°C incubator they grew much more rapidly and were converted to the mycelial phase.

Growth from a 6-day-old blood agar culture of the yeastlike form of the isolate was suspended in 3 ml of saline, and 0.5 ml of this suspension was injected intravenously into the tail veins of three adult mice. One of the mice died on the 7th day, the other two on the 8th. At autopsy the lungs of all three mice showed a massive embolic pneumonia with consolidation into a firm, gray, granulomatous mass. Direct examination of all portions of the lungs revealed numerous tissue-phase cells of *B. dermatitidis*. Cultures made of the livers and spleens, as well as of the lungs, were positive for *B. dematitidis*. The fungus grew in the yeast phase at 37°C and the mycelial phase at 25°C.

The virulence of 0.5 ml of a suspension of the yeastlike form, which showed 90 percent light transmission at a wavelength of 550 mμ on a Spectronic 20 colorimeter when it was injected intravenously, was compared with six other isolates of *B. dermatitidis*—five from human beings and one from the northern sea lion (*6*). The soil isolate was found to be the most virulent.

The positive sample was taken from the earth floor of a stripping shed attached to a tobacco barn located near Lexington, Kentucky. A wooden bench about 3 feet wide was attached to the wall along one side of the room. A stove completed the furniture. Fragments of tobacco left from the stripping process had accumulated on the ground, especially under the bench. The soil that yielded *B. dermatitidis* was taken from the floor just inside the door, at the narrow end of the room. This sample, a brown clay loam, contained little gross organic matter and had a *p*H of 5.8 as determined by a Beckman *p*H meter.

This shed at one time had been used to shelter a dog affected with blastomycosis. In December 1955, a diagnosis of blastomycosis was made in a female Weimaraner approximately 1 year old, living at a house in a nearby town. For much of the time, from the diagnosis of its infection until its death in July 1956, this dog was housed in the shed from which the positive soil was collected. She is reported to have bedded down often near the door and under the bench. Since the dog exuded purulent

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

material from many abscesses scattered over the feet and body, she had ample opportunity to inoculate the soil. However, from the time of her death in July 1956 until the sample was collected on 8 October 1958, there was no known subsequent contamination of the shed with *B. dermatitidis*.

Although *B. dermatitidis* was isolated from only a single soil specimen, it has been demonstrated for the first time that this fungus can and does exist in soil under natural conditions for a long period of time. Success in isolating it when numerous attempts by others, as well as ourselves, had failed is thought to be due to the improved technique employed. It is the general opinion, with some supporting evidence, that the laboratory mouse is not especially susceptible to infection with *B. dermatitidis* when the organism is inoculated intraperitoneally. However, Heilman (*7*) found that intravenous inoculation of small numbers of both the yeastlike and mycelial forms caused death, while intraperitoneal injection of comparable doses of the same strains did not cause any symptoms in the mice. Inoculation of *B. dermatitidis* by the intravenous route allows the organisms to be carried directly to the lungs, which appear to be the most favorable site for infection to develop (*8*). Whether or not the addition of *Mycobacterium fortuitum* enhanced the isolation of *Blastomyces dermatitidis* is not clear at this time (*9*).

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Ability of Some Black Sea Organisms To Accumulate Fission Products

Abstract. The coefficients of accumulation of strontium-90, cesium-137, and cerium-144 in seaweeds, eelgrass, actinia, mollusks, and crustaceans are presented. The discharge of strontium-90 into sea water from decomposing seaweed and the retention and additional absorption of cesium-137 and cerium-144 onto organic debris is discussed. Some observations are made about the ability of these elements to diffuse into sea water and about the relative hazard to man from strontium-90 and cerium-144 in marine life.

A great deal of importance is attached to the elucidation of the role of living organisms in the general problem of the diffusion of fission products into ocean and sea waters (*1, 2*). This suggests the need for an investigation of the ability of different sea animals and plant life to accumulate the most important radioactive substances (*3, 4, 5*).

The following different marine plants and animals were selected for investigation in the present study: green algae (*Ulva rigida, Enteromorpha minor*), brown algae (*Dictyota fasciola, Padina pavonia, Cystoseira barbata*), red algae (*Corallina rubens, Ceramium rubrum, Polysiphonia elongata, Phyllophora rubens, Laurencia obtusa*), eelgrass (*Zostera marina*), coelenterata (*Actinia equina*), mollusks (*Mytilus galloprovincialis*), and crustaceans (*Pachygrapsus marmoratus, Carcinus moenas, Leander squilla*). The concentrations of Sr^{90} (in 15 species), Cs^{137} (in 7 species), and Ce^{144} (in 12 species) in these marine organisms were studied.

Specimens of the various organisms were placed in separate glass vessels filled with 2 to 5 liters of filtered sea water which contained concentrations (10μc/lit.) of Sr^{90}, Cs^{137}, and Ce^{144} (solutions of chlorides). Samples of water and organisms were collected simultaneously after 3, 6, and 12 hours and 1, 2, 4, 8, 16, 32, and 64 days had elapsed after the start of the study. One-milliliter samples of the sea water, placed in standard aluminum planchets, were evaporated under an infrared lamp. The samples of plant and animal organisms were rinsed in clear sea water, dried on filter paper, weighed immediately, and then placed in drying ovens at 80° to 90°C, after which their dried constant weight was determined. The desiccated organisms were powdered, and 10-mg paired samples of the powder were placed in standard planchets for counting. There was no self-absorption of radiation in these samples. Correction for radioactive decay was introduced for samples with Ce^{144}. The counting of Sr^{90} in the samples was done only after Sr^{90} and Y^{90} had reached equilibrium. Radiation was measured with a Geiger-Müller type counter, with a possible error of 3 percent.

The mean values of the coefficients of accumulation (the ratio of radioactivity in the organism to that of an equal weight of water) were calculated by averaging a number of coefficients, beginning with the samples of Sr^{90} collected on the second day, and with samples of Cs^{137} and Ce^{144} on the eighth day, after the experiment began. This was because from this time on (occasionally even earlier) these values were practically constant in all samples. One exception was the uninterrupted increase of Sr^{90} accumulation in the crustaceans and in the mollusk (*Mytilus*) shells. In these two, only the final values—that is, the highest coefficients of accumulation—were considered. The studies continued for as long as the organisms remained alive. The coefficients of accumulation of Sr^{90} for the crustaceans were studied through the 8th-day samples, and for the mollusk shells through the 64th day. The coefficients for Cs^{137} and Ce^{144} in the crustaceans and the mollusk shells were calculated through the 16th-day samples.

The coefficients of accumulated values for Sr^{90}, Cs^{137}, and Ce^{144} for certain species of marine organisms are shown in Table 1. The data for the coefficients of accumulation of Sr^{90} in

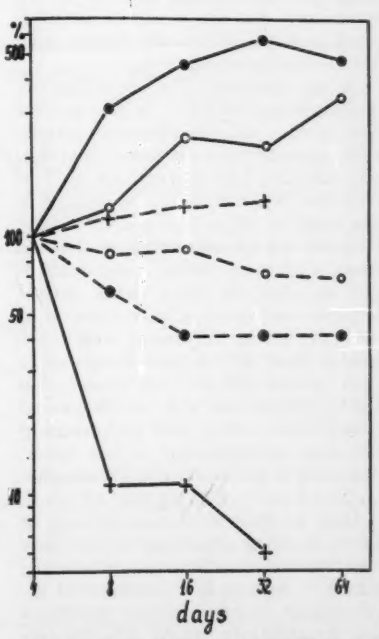

Fig. 1. Change of values of coefficients of accumulation (solid lines) of Sr^{90}(+), Cs^{137}(○), and Ce^{144}(●) in the process of dying off of *Cystoseira*. Concentration of activity in sea water is shown by dashed lines. The times are from the beginning of the experiment. Dying began after 4 days in the absence of fresh sea water.

Table 1. Coefficients of accumulation of Sr^{90}, Cs^{137}, and Ce^{144} in some marine organisms.

Specimen	Coefficients of accumulation					
	Living weight			Dried weight		
	Sr^{90}	Cs^{137}	Ce^{144}	Sr^{90}	Cs^{137}	Ce^{144}
Green and red algae	1–6	4–10	300–900	10–30	40–50	2600–4500
Brown algae	20–40	30	340	150–280	200	2500
Zostera	3	2	180	30	20	1800
Actinia	1	10	165	9	60	1500
Mytilus						
Soft body	0.7	10	360	4	80	2600
Shells	6	0	43	6	0	43
Total	6	3	33	6	20	60
Crustacea	3–8	10	230	10–35	30	670

algae and mollusks of the Black Sea are in agreement with the results of similar studies on algae in the Atlantic Ocean (*3*) and oysters in the Pacific (*4*).

After the death of the organism, and in the process of decomposition of the algae *Cystoseira barbata,* the accumulated Sr^{90} returns to the sea water (Fig. 1), and the values of the coefficients of accumulation fall, approximating unity. The same is true for other brown algae. The coefficients of accumulation for Sr^{90} in green and red algae do not change at the time of the death of these organisms, but remain near unity. Cesium-137 and Ce^{144} not only remain in the amount which had been absorbed during the life of the organisms, but are absorbed in additional quantities from the solution onto the remaining debris (Fig. 1).

If we designate the coefficients of accumulation for Ce^{144} as K_{Ce} and for Sr^{90} as K_{Sr}, and the maximum permissible concentration in water for man (*6*) for Ce^{144} as C_{Ce} and for Sr^{90} as C_{Sr}, then $K = K_{Ce}/K_{Sr}$. K will be in the order of 10 to 100; and $C = C_{Ce}/C_{Sr}$ will be in the order of 10,000; hence C/K gives values in the order of 100 to 1000. In other words, despite the low coefficients of accumulation of Sr^{90} in marine organisms which are used as food, Sr^{90} is more dangerous to man (about 100 to 1000 times) than Ce^{144}, which has high coefficients of accumulation, when both are present in the same concentration in sea water. The same is apparently true for all other mammals which feed on sea organisms.

High coefficients of accumulation of Ce^{144} in living organisms and in their organic remains indicate it is less mobile than Sr^{90} with its low coefficient of accumulation in mass bottom organisms and its ability to return into the seawater (when the coefficients of accumulation are greater than 1). Cesium-137 occupies an intermediate position. However, there is a possibility of Ce^{144} being retained in the upper layers of the ocean by plankton (*7*). Strontium-90 is a constant source of Y^{90}, which has high coefficients of accumulation in marine organisms (*5*). By using the concept of zones of accumulation of radioactivity in water reservoirs (*8*), it is possible to classify Sr^{90}, in contrast to Ce^{144}, in the group of elements not forming a distinct zone of polyaccumulation. Obviously it is necessary to take into consideration these characteristics of Sr^{90}, and also the most recent oceanographic information (*1, 9, 10*), when one is discussing problems of disposal of highly radioactive wastes of the nuclear industry into the depths of the sea, and the Black Sea in particular. According to the latest data (*10*), the time interval for the rise of bottom waters to the surface in the Black Sea is from 60 to 130 years. In the course of this time period Sr^{90} radioactivity would diminish 5 to 30 times.

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23 November 1960

Editor's note: Translation from the Russian was done at our request by Mrs. R. G. Ernsberger of the translating unit of the National Institutes of Health, Bethesda, Md. The English version was approved by the author.

Slowing of Heart Rate after Septal Self-Stimulation in Rats

Abstract. Heart rate, recorded continuously from rats trained to press a bar for intracranial electrical stimulation of their septal areas, fell consistently after brain stimulation. Interpretation of the rewarding effect of septal stimulation had previously been limited by the absence of any data on the autonomic effects of the stimulation. The results of this study suggest that the rewarding effect may possibly be produced by a parasympathetic (quieting) reaction of the autonomic nervous system to septal stimulation.

Heart-rate recording was chosen as a first step in the physiological investigation of intracranial self-stimulation (i) because of the clear way in which it reflects the balance between sympathetic and parasympathetic influences, (ii) because a method for recording the heart rate of the free-roving rat was available, and (iii) because Hess's previous studies of septal stimulation in the cat had revealed slowing of the heart rate (*1*).

Electrodes were implanted in the brains of seven 225-g male hooded rats by the method previously described by Olds and Milner (*2*). Michel wound clips with soldered bus-wire attachments were used as electrocardiograph electrodes. A recovery period of at least 3 days was allowed after operation before we started the two habituation sessions and the five subsequent 20-minute experimental sessions in the testing apparatus (Skinner box), all on consecutive days. The testing apparatus was similar to that previously described (*2*). A lever placed near the floor of the box actuated a microswitch in the stimulating circuit, so that by pressing it the rat received electrical stimulation delivered by a Grass model S4 stimulator set for biphasic electrical stimulation with a duration of 0.5 msec and a frequency of 100 cy/sec. Voltage (peak-to-peak) was set initially at 1.5 volts and was advanced as required, usually to 5 to 7 volts. A Hunter timer was used in the circuit to cut the current off after a period of 0.5 second if the rat continued to hold the lever down. In the initial stage when the experimenter was teaching the animal to self-stimulate, the experimenter stimulated the animal by activating the bar-depression mechanism from outside the test box. In the latter half of the fifth experimental session the current was turned off so that when the animal pressed the bar there was no electrical stimulation; this was the extinction procedure.

During experimental sessions, continuous tracings of electrocardiograms and bar-presses were taken on a Grass model 5A Polygraph, Microdot "Mininoise" triaxial cable (No. 50-3928)

being used at the preamplifier end for electrocardiograph recording.

Figure 1 is a photomicrograph showing the electrode track in a stained and mounted section from the brain of subject 5 at the level of the anterior commissure. This electrode placement appeared favorable for producing high frequency of self-stimulation and very marked and consistent heart-rate slowing. The upper part of Fig. 2 is a photograph of the electrocardiographic tracing from the first part of the fifth session. Note the slowing of heart rate associated with self-stimulation (6 volts), and also the increased amplitude of the R wave. Both phenomena were typical of the rather gross electrocardiographic changes regularly observed after self-stimulation in this animal. Systematic quantitative analysis of the heart-rate data for this animal showed that mean heart rate for periods with self-stimulation was 442.2 beats per minute compared with 389.4 beats per minute for preceding periods without intracranial stimulation. Corresponding mean values for subjects 6 and 10 that also had the five full-training sessions were 451.2 and 442.8 for subject 6, and 428.4 and 400.8 for subject 10. All of these differences were highly significant statistically ($p < .001$).

The lower part of Fig. 2 shows the heart-rate deceleration produced in subject 7 by the experimenter's presentation of a 7.5-volt stimulation to the septal area. Note the skipped beat. This marked slowing with skipped beat was a reproducible phenomenon in this animal, just as it was in subject 8. Data from two training sessions were available for analysis in these two animals. For subject 7 mean heart rate during stimulation by the experimenter was 349.2 beats per minute compared with a mean of 414.6 beats per minute for the periods preceding stimulations. Corresponding mean values for subject 8 were 328.2 and 363.0 beats per minute, and again the differences were significant at the .001 level of confidence. In addition, the heart-rate slowing produced in subjects 7 and 8 by their own self-stimulation was highly consistent and was very reliable statistically.

For the three animals carried through to extinction, percentage bar-pressing [computed by the method of Olds and Milner (*2*, p. 421)] was from three to five times less than the percentage during reinforcement. Subject 5, the animal with the highest bar-pressing percentage (89.2), was also the animal showing the most striking heart-rate responses to stimulation (gross slowing in 31 consecutive self-stimulations). Comparison of histological findings for this animal (see Fig. 1) with those for

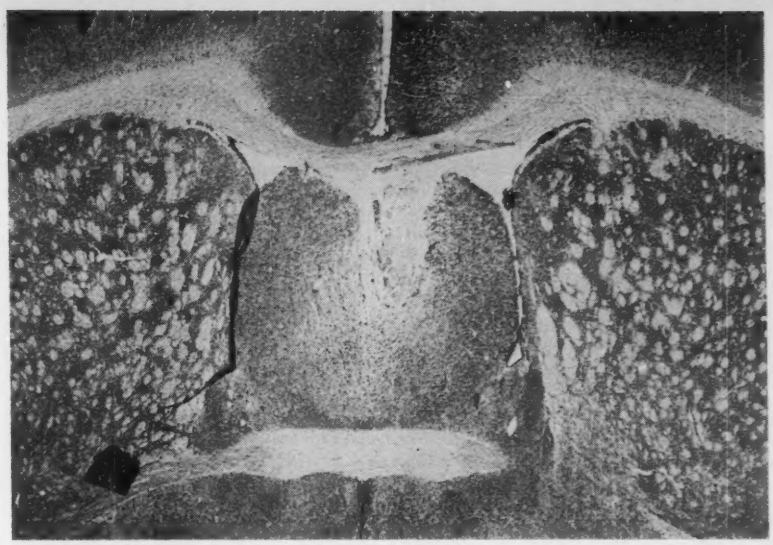

Fig. 1. Part of the electrode track in a brain section stained with cresyl-violet. The track made by the stimulating tip of the electrode is in the septal area to the right of the midline in this section which is at the level of the anterior commissure.

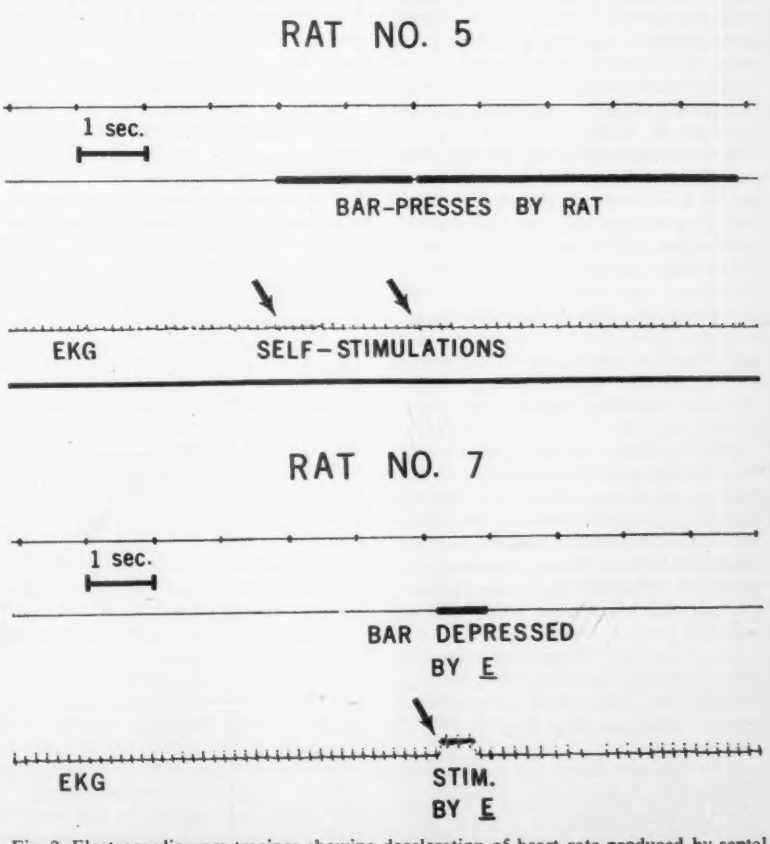

Fig. 2. Electrocardiogram tracings showing deceleration of heart rate produced by septal self-stimulation in animal 5, and by septal experimenter-produced (*E*) stimulation in the brain of animal 7. Arrows indicate the onset of stimulation, and duration of stimulation is shown by the artifact in the tracing. Other switching artifacts in the tracing occurred with onset and termination of bar-pressing. The top line is a time marker.

other animals showed that the electrode track had penetrated deeper into the septal area of this animal than it had in the others. This animal, like the others, showed no habituation of the heart-rate slowing response. No statistically significant change in heart rate was observed during extinction in any of the animals. Subject 10 pressed only briefly with relatively long intervals of nonpressing between bar-presses (acquisition and extinction percentages were 15.0 and 2.7, respectively, as compared with percentages of 40.7 and 12.6 for subject 6). Histological findings for subject 3, dropped from the experiment after one session for lack of promise, showed failure of the electrode to penetrate through the *corpus callosum* into the septal area. In subject 4, when experimenter-produced and self-stimulations from the one available session were combined, the difference between heart rate immediately before and immediately after stimulation was a statistically reliable one (again in the direction of heart-rate slowing after stimulation). In summary, all six animals with positive histological findings (that is, electrode track in the septal area) showed significant slowing of heart rate upon stimulation.

The results clearly show that septal stimulation, whether self-produced or presented by the experimenter, had the very consistent effect of slowing the rate at which the heart was beating. As far as heart rate is concerned, therefore, septal stimulation produced a parasympathetic or quieting effect with reinforcing properties. This finding is of interest in relation to the result of the study by Brady and Nauta (*3*) which, as Olds and Milner noted, "suggests that the septal area is a quieting system, for its surgical removal produced an extremely active animal" (*2*, p. 426).

The fact that a consistent, objectively recordable physiological change of this kind occurs in association with the act of septal self-stimulation is encouraging with respect to further experimental inquiries into the nature of reinforcement produced by intracranial stimulation, and into the problem of reinforcement generally (*4*).

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Fallout from Nuclear Detonations of February and April 1960

Abstract. A sharp increase in the ratio of strontium-89 to strontium-90 in rain was observed at Fayetteville, Arkansas, after the French nuclear detonations of February and April 1960. Experimental data obtained suggest the possibility that part of the debris from atom bombs detonated in the tropical region may enter the stratosphere.

It is generally accepted that atom bombs, such as those detonated at Reggan in the Sahara Desert on 13 February and on 1 April 1960, inject their debris solely into the troposphere, whereas debris from hydrogen bombs enter the stratosphere and cause the world-wide stratospheric fallout.

Although the French nuclear detonations caused seemingly small transient increases in fallout and the fresh debris will most likely add no more than a few tenths of a percent to the total world-wide fallout of long-lived fission products, the radioisotope injection into the atmosphere by the French nuclear detonations was of unique scientific interest in that the nuclear explosions occurred in the tropical region after a long suspension period of nuclear testing.

According to the global circulation model of air masses, proposed by Brewer and by Dobson (*1*), there is an upward flow of air in the tropical region, and hence a material transfer from the troposphere to the stratosphere is expected to occur. Thus, part of the debris from the atom bombs detonated in the tropical region may enter the stratosphere.

Kuroda and his co-workers (*2*) reported the results of the measurements of the ratios of radioisotopes in rain and snow collected at Fayetteville, Ark., during the nuclear test suspension period, which lasted for approximately 16 months after November 1958. As a result of these studies, we have obtained fairly accurate knowledge of the relative ratios of a number of fission products in the stratosphere prior to the French nuclear detonations.

We have continued the measurements of the barium-140, strontium-89, and strontium-90 contents in individual samples of rain and snow collected at Fayetteville, Ark. Radiochemical procedures used were essentially the same as those described by Kuroda (*3*). Because of the extremely small concentrations of the fission products in some of the recent rain samples, usually

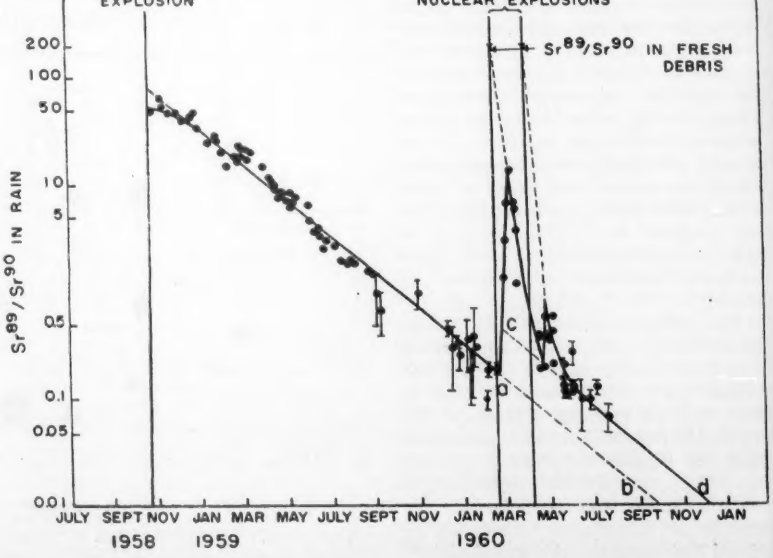

Fig. 1. Variation of the ratio of strontium-89 to strontium-90 in individual samples of rain and snow collected at Fayetteville, Arkansas, since November 1958.

4, but sometimes 8 liters of samples were taken for the radiochemical analyses.

Figure 1 shows the variation of the Sr^{89}/Sr^{90} ratio in rain. A sharp increase in the ratio was observed 10 days after the first French nuclear explosion, and the ratio reached a maximum approximately 3 weeks after the explosion. The second peak which followed the April explosion was small and not too well defined.

Results from our previous studies predicted that the Sr^{89}/Sr^{90} ratio in the stratosphere after February 1960 would have been something like the dotted straight line *ab* shown in Fig. 1, if the February and April nuclear explosions had not taken place.

If the debris from the French nuclear detonations did not enter the stratosphere, the Sr^{89}/Sr^{90} ratio in rain should have asymptotically approached the straight line *ab* 1 to 2 months after the April nuclear explosion. Figure 1 indicates, however, that a new Sr^{89}/Sr^{90} ratio, given by the straight line *cd*, was established in the stratosphere after the French nuclear detonations. This can be explained by the fact that some of the Sr^{89} and Sr^{90} atoms produced by the French nuclear explosions were transported from the troposphere to the stratosphere by the upward flow of air in the tropical region, described by Brewer and by Dobson (*1*).

It is possible to make a rough estimate of the increase in the stratospheric Sr^{90} inventory due to the French nuclear explosions. If it is assumed that the February explosion was chiefly responsible for the increase in the stratospheric Sr^{90} inventory, the Sr^{89}/Sr^{90} ratio in the stratosphere has increased from 0.15 ± 0.05 (curie/curie) to 0.5 ± 0.1 (curie/curie) because of this nuclear explosion. It is known that the fissionable material used in the bomb was plutonium. The Sr^{89}/Sr^{90} ratio in a fresh fission products mixture from a plutonium bomb explosion can be calculated to be approximately 170 (curie/curie), and hence

$$\frac{(0.15 \pm 0.05)P + 170\,Q}{P + Q} = (0.5 \pm 0.1) \qquad (1)$$

where P is the total quantity of Sr^{90} in the stratosphere prior to the nuclear explosion and Q is the quantity of Sr^{90} which entered the stratosphere from the French nuclear detonation. Solving Eq. 1 for Q/P, a value

$$Q/P = 0.0021 \pm 0.0009$$

is obtained.

It is possible that the April explosion was chiefly responsible for the increase in the stratospheric Sr^{90} inventory, and similar calculation yields a value

$$Q/P = 0.0010 \pm 0.0003$$

for this case. Thus, it would appear that the percentage increase in the Sr^{90} inventory of the Northern Hemisphere due to the French nuclear explosions was approximately 0.1 to 0.2 percent (*4*).

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* On leave from the department of chemistry, Arkansas State College, State College, during the summer of 1960.

7 November 1960

Blockade of Deoxyribonucleic Acid Synthesis by Deuterium Oxide

Abstract. Interference with deoxyribonucleic acid replication need not be a *primary* mechanism in the blockade of cell division by deuterium oxide, but current hypotheses on the molecular basis of the blockade do suggest that such interference might take place under appropriate conditions. Autoradiographic experiments support the suggestion, for whereas normal sea urchin eggs incorporate H^3-thymidine into deoxyribonucleic acid from almost the beginning of development, cells immersed in deuterium-enriched media do not. Blockade of mitosis and inhibition of thymidine incorporation are simultaneously relieved when the eggs are returned to normal water.

Disturbances of cell division are the most obvious of the growth-inhibiting effects of heavy water on microorganisms. These effects have been the objects of several investigations (*1*). Katz and his co-workers have been able to adapt algae to growth in 99+ percent D_2O, and Flaumenhaft *et al.* observed changes in the patterns of nucleic acid synthesis and distribution in deuterated *Chlorella* and *Scenedesmus* (*2*). Serious disturbances of cell division follow also upon exposure of higher organisms to D_2O (*3*). Gross and Spindel (*4*) have found that D_2O inhibits reversibly the mitotic division of marine invertebrate eggs, and that the inhibition can be imposed at any stage, including even cytokinesis. Their interpretation of the data emphasizes a cooperative deuterium isotope effect upon the operation of multiple hydrogen-bonded structures in the cytoplasm, among them the mitotic apparatus and the cleavage furrows.

Calvin and his associates have supposed that cooperative effects of this type might appear at the molecular level, importantly so in the synthesis and function of proteins and nucleic acids (*5*). Since mitotic inhibition by D_2O can be effected in the sea urchin egg *after* deoxyribonucleic acid (DNA) replication is complete, and indeed long after metaphase (*4*), such a mechanism cannot alone account for the mitotic block. Yet cytochemical evidence does suggest alterations of nucleic acid synthesis in deuterated algae (*2*), and it is therefore of interest to determine whether DNA synthesis is affected by deuteration of cells other than microorganisms.

For theoretical and practical reasons (*4*), the sea urchin egg has been chosen for this work. A test of the hypothesis that DNA synthesis is affected by deuteration is afforded by autoradiography of cells exposed to tritiated thymidine during the period of DNA replication. Harding and Hughes (*6*) and Bucher and Mazia (*7*) have already shown that H^3-thymidine is incorporated into polymers during the early cleavages of sea urchin eggs, and the temporal relations between isotope incorporation and the appearance of mitotic figures have most recently been studied in injured lens epithelium by Harding and Srinivasan (*8*).

Eggs and sperm of *Arbacia punctulata* were obtained in the usual way. A large population of eggs (with jelly coats removed) was inseminated in normal sea water at 21.5°C. Exactly 5 minutes later, when 95+ percent of the cells had elevated normal fertilization membranes, the population was divided, half of the cells being placed in filtered sea water containing H^3-thymidine and the other half in a reconstituted deuterium sea water (90+ percent of D) containing H^3-thymidine. The thymidine had a specific activity of 1.9 c/m mole and was present in both incubation media at a level of 8.33 μc/ml.

Samples were removed from the cultures to acetic-alcohol and dilute formalin fixatives at brief intervals, beginning with the time when the control eggs (in normal sea water and thymidine) reached 50 percent first cleavage. One hundred and eight minutes after fertilization, when the controls were dividing from four to eight cells (the deuterated cells still undivided), the D_2O-treated population was freed of excess deuterium by repeated centrifuga-

tion through the normal sea water–thymidine medium. The cells remained therein until the experiment was terminated. Samples were observed and photographed throughout, these procedures serving to determine the cleavage intervals, the type and severity of mitotic inhibition, reversal thereof upon removal of D_2O, and the general morphology of the embryos. To the latter end, the cultures (both now in normal sea water–thymidine) were permitted to remain on the water-table overnight at 21.5°C.

Fixed cells were prepared for autoradiography by sectioning of paraffin-embedded material and by mounting of the sections on gelatin-coated slides. These were cleared and then covered with Kodak AR-10 stripping film. Exposures were for periods of 7 to 18 days, and the emulsions were developed without staining of the sections. Observations were made by means of bright-field and phase-contrast microscopy.

Eggs which had been placed in normal sea water–thymidine 5 minutes after fertilization cleaved normally at 57 minutes. The second division appeared on schedule. Cells of this population produced, ultimately, normal-appearing, free-swimming larvae. No significant deviations from the ordinary course of development were observed. This is surprising in view of the large dose of radiation supplied directly to the genetic material almost from the beginning of development (compare Harding and Hughes, *6*).

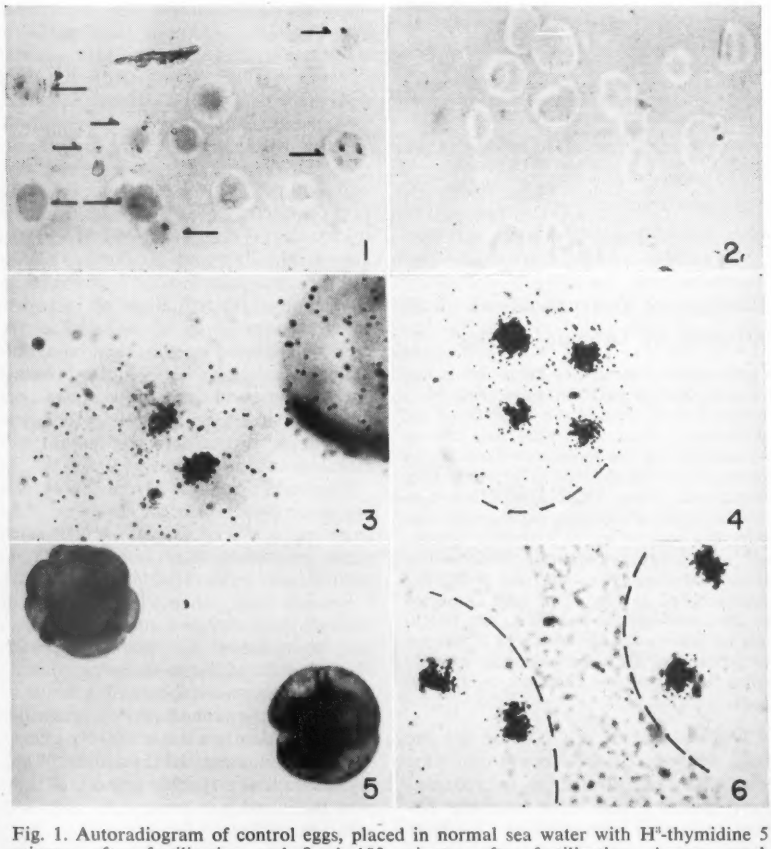

Fig. 1. Autoradiogram of control eggs, placed in normal sea water with H^3-thymidine 5 minutes after fertilization and fixed 103 minutes after fertilization. Arrows mark sections with labeled nuclei. Fig. 2. Autoradiogram of sectioned eggs, treated like the controls, except that the medium contained 90+ percent D_2O. No radioactivity above background. Fig. 3. High-power photomicrograph of sections from the control cell population. Unstained, bright-field illumination, focus on the emulsion grain. Two nuclei labeled. Fig. 4. High-power photomicrograph of sections from control cell population, fixed 149 minutes after fertilization. Dashed line indicates cell border. Four labeled nuclei. Fig. 5. Cells placed in sea water with 90+ percent D_2O and H^3-thymidine 5 minutes after fertilization, then washed free of excess deuterium after 108 minutes. Shown are two living cells from this population, 149 minutes after fertilization, with multiple cleavages. Fig. 6. High-power photomicrograph of sections from eggs shown in Fig. 5. Cells were not labeled during their 108 minutes in D_2O-thymidine, but 41 minutes after removal of D_2O, all nuclei in blastomeres resulting from the multiple cleavages were radioactive. Dense material between cells is an inert protein binder.

The deuterated cells did not divide at all during their sojourn in heavy water and thymidine. At 108 minutes after fertilization, they were washed with, and placed in, normal sea water–thymidine. Thirty-seven minutes later most of the cells had divided, and the divisions were mainly multipolar. It is difficult to be certain that the multiple cleavages observed were truly simultaneous, but, in any case, cytokinetic activity in these cells accomplished in 37 minutes what had required more than 100 minutes in the controls.

Examination of the "reversed" deuterium culture after an overnight incubation revealed that, whereas most of the cells had continued to divide, little or no differentiation had taken place, so that except for a few scattered swimming forms the 'embryos" were multicellular masses lying inert on the bottom of the culture dish. Thus the cell division (or cytokinetic) mechanism survived the period of deuteration, but irreparable damage had been done to the sources of information for differentiation.

The numerous peripheral clear zones which appeared in the deuterated cells were no longer visible after "reversal," but in fixed (unwashed) material stained with hematoxylin, the "clear zones" (referred to as "mottling" in an earlier paper, *9*) resembled cytasters. Perhaps the multiple furrowing of the deuterium-treated cells is related to the appearance of these structures, a possibility which would weaken a previous supposition (*4*, *9*) concerning the continuance of synthetic activity in cells immersed in D_2O.

Cells maintained in normal sea water and thymidine were already labeled when they were first sampled, that is, at 50 percent first cleavage. The label was in the nuclei when present and in mitotic figures when these were included in the section. *No isotopic incorporation* took place in the deuterated cells. Figure 1 shows a typical low-power field of control cells sampled at 103 minutes after fertilization. The cells were at this time entering the second division. The microscope was focused on the emulsion grain, and groups of grains appear as black deposits. Of the 14 sections in the field, eight contain nuclei or mitotic figures, and all such are labeled. Figure 2 is a low-power photomicrograph of sections of deuterated cells sampled at the same time as those in Fig. 1. Among the ten full-diameter sections shown, at least five include nuclei, but none show radioactivity above background.

Figures 3 and 4 show sections, at high power, of the control cells. Those in Fig. 3 were sampled 103 minutes after fertilization. The two nuclei are strongly labeled. Figure 4 is a section

of a control cell fixed 149 minutes after fertilization, a time when the population was entering the fourth division. The cell shown in the figure had evidently been sectioned in a plane including four nuclei, each of which was labeled, as evidenced by the accumulation of silver grains.

The multiple cleavages in the cells from which the D_2O had been removed are shown in vivo in Fig. 5, and an autoradiogram of the same material in section is shown in Fig. 6. *The nuclei are now labeled.* The sections shown in the figure include two nuclei each, and all four of these exposed the emulsion strongly. Thus, the release of the block to cell division releases as well the block to the synthesis of DNA. It is not yet possible to conclude that this rather remarkable "switching-on" of DNA synthesis is matched by an equally abrupt "switching-off" upon deuteration, since we do not know whether or not synthesis had begun when the eggs were immersed in D_2O. Because the pronuclei had not yet fused, it is possible that DNA replication had in fact not begun, which would make the mechanism of action of D_2O in the present case an inhibition of the *onset* of synthesis, rather than the blockade of a process already in full career. These qualifications are mitigated, however, by the interesting finding of Bucher and Mazia (*7*) that H^3-thymidine is incorporated into the DNA of *Strongylocentrotus purpuratus* eggs whether the pronuclei have fused or not.

Failure of the H^3-thymidine to penetrate the deuterated cells, rather than any chemical isotope effect, might explain these results. Such an absolute permeability differential for normal and deuterated sea urchin eggs is rather unlikely, but the possibility does require experimental study (*10*).

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29 December 1960

Bulbar Control of Cortical Arousal

Abstract. In the cat, the cortical arousal induced by reticular or nociceptive stimulation is more intense and more long-lasting after elimination of the caudal and medial part of the medulla than in preparations with intact brain. This difference is explained by the intervention of a phasic-ascending inhibitory bulbar control secondarily triggered by the mesencephalic activating system.

We have recently published experimental data which led to the conclusion that a brief stimulation of the mesencephalic reticular facilitating system is able to set secondarily into action a phasic-descending bulbar inhibitory control. This control was evidenced by the decreased amplitude of a sympathetic response, the galvanic skin response recorded on the cat's foot pad (*1*). During these experiments, we noticed that the depression of the sympathetic response was accompanied by decreased intensity of the cortical response to the same mesencephalic stimulation. Because of the simultaneity of both these descending and ascending effects which follow reticular stimulation, we assumed that the decrease of the cortical response might also be the consequence of a bulbar control mechanism. The present investigation was designed to test this hypothesis of bulbar control of cortical arousal.

The experiments were performed on acute spinal cats (cord section at T_2 level), which had been operated under ether. A few hours after the surgical procedure, the preparations were paralyzed with Flaxedil, artificially ventilated and slightly nembutalized (3 to 7 mg/kg). This narcosis, by depressing the normally predominant reticular activating influence, facilitates the "unmasking" of coexisting inhibitory processes. The spontaneous electrical cortical activity of such preparations shows sleep patterns (see Fig. 1); a cortical arousal, however, may easily be evoked by the stimulation of the mesencephalic reticular activating system or by the stimulation of an afferent nerve of the anterior limb (median, cubital, or superficial radial nerve). Because of the spinal section, these stimuli do not affect the cardiovascular sympathetic tone.

The electroencephalographic responses to reticular or afferent stimulation have been compared in preparations (i) with the intact brain and (ii) after elimination of the effects of the bulbar inhibitory formation (*2*) by either a prebulbar (retropontine) tran-

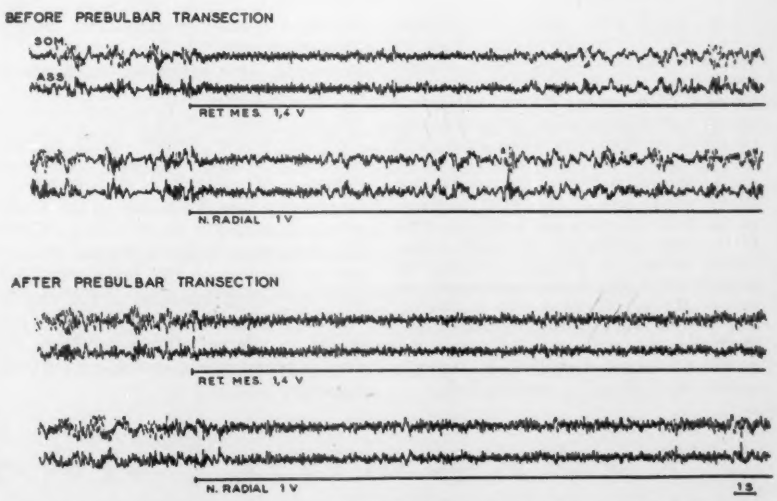

Fig. 1. Cat; spinal cord section at T_3; Nembutal, 7 mg/kg. Effect of a medial prebulbar section of the brain stem on the duration of the cortical arousal induced by long-lasting reticular (mesencephalic) and afferent (superficial radial nerve) stimulation. Before the section (upper traces) the arousal is short and soon followed by the return of the initial sleep pattern. After the section (lower traces), the arousal remains sustained as long as the stimuli are applied.

section or by novocainization of the caudal medulla.

In preparations with the intact brain, short stimulation (0.2 to 0.5 second) of the mesencephalic reticular formation induces a brief and intense burst of cortical activation, but an identical stimulus applied a few seconds later results in a second cortical response which is less intense and much shorter than the first one. In the same preparations, however, after a prebulbar brainstem transection, the two cortical bursts of activation produced by two consecutive reticular stimuli are of the same intensity and duration.

These observations prove that in the intact preparation the depression of the second cortical response is not due to a reticular "fatigue," but to an active inhibitory mechanism lying below the transection and set in action by stimulation of the midbrain reticular formation.

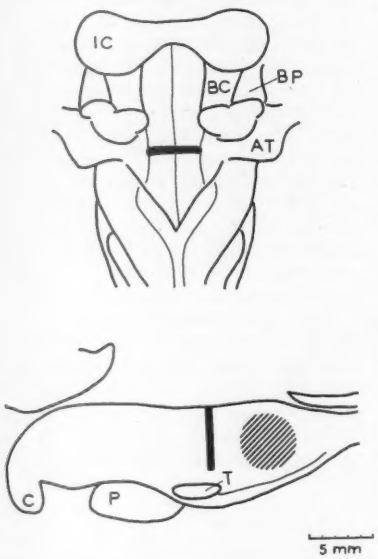

Fig. 2. Semischematic drawings of the lower brain stem of the cat, showing the location of a medial bulbar transection and of a local injection of novocaine, both of which suppress the bulbar control of cortical arousal. In the upper diagram the level and width of the section are shown on the floor of the fourth ventricle. In the lower diagram, the level and depth of the same section are shown in a midsagittal section. In the same diagram, the hatched circle shows the approximate extent of the novocainized area in another preparation. *AT*, Acoustic tubercule; *BC*, brachium conjunctivum; *BP*, brachium pontis; *C*, corpus mamillare; *IC*, inferior colliculus; *P*, pons; *T*, trapezoid body.

In the preparations with the intact brain, a sudden and long-lasting stimulation of either the mesencephalic reticular formation or a somesthetic nerve provokes at its onset a cortical arousal. Progressively however, in spite of the maintenance of the stimulus, the frequency of the cortical waves decreases and spindle bursts and slow waves appear on the electroencephalogram (Fig. 1, two upper traces). When low stimulation voltages (0.5 to 1 v) are used, the arousal is short and the cortical deactivation is usually completed in less than 20 to 40 seconds. After a medial prebulbar transection sparing the lemniscal pathways, or after a local injection of novocaine in the ventro-caudal part of the medulla (see Fig. 2), the arousal induced by the same stimuli persists unchanged throughout the duration of the stimulation (Fig. 1, two lower traces). In many cases a typical activation pattern was still present after 3 or 4 minutes of reticular or afferent stimulation. These results indicate that an ascending bulbar mechanism is able to antagonize the cortical arousing effect of a sustained reticular excitation, but that this mechanism reacts with some inertia and is only progressively brought into action.

The inhibitory mechanism may be more easily activated if repetitive stimuli are used, the voltage of which is linearly increased in 1 or 2 minutes from zero to the voltage normally sufficient to provoke a typical arousal. In such a case, it frequently occurs that the cortical activity remains synchronized during the whole duration of the stimulation, even when a voltage is reached that is sufficient to induce an intense arousal when applied suddenly. It may be shown that the difficulty of evoking a cortical activation in this case is not due to the mode of stimulation but to an active inhibitory discharge. After a medial prebulbar transection of the brain stem, or after the novocainization of the caudal medulla, progressively increasing reticular or afferent stimuli regularly induce a progressively increasing cortical activation.

Although most of this experimentation was performed on lightly nembutalized preparations, observations identical to those described above were also made by stimulating the reticular mesencephalic formation in a few "encephale isolé" unanesthetized cats, whose cortical activity was spontaneously of the drowsy type. Thus the bulbar inhibitory mechanism exerts its control under physiological conditions.

These experiments suggest the following considerations:

The foregoing results show that structures localized in the medial and caudal part of the medulla are able to decrease or suppress the arousing influence of the excitation of the reticular activating system. It seems highly probable that these structures are the same which are at the origin of the tonic synchronizing influence recently described by Moruzzi and his colleagues (*3*).

The present investigation proves, on the other hand, that the bulbar-ascending deactivating control is phasically brought into action each time the reticular activating system is activated. Our previous experiments (*1*) have demonstrated that this is also the case for the descending bulbar inhibitory discharges, even when the supramesencephalic structures are eliminated. Taken together, these data suggest that it is the reticular activating system itself which triggers the reticular bulbar antagonistic mechanism. Depending upon whether the stimuli are abrupt or gradual, this interreticular regulating mechanism may either shorten or completely suppress the cortical activation (*4*).

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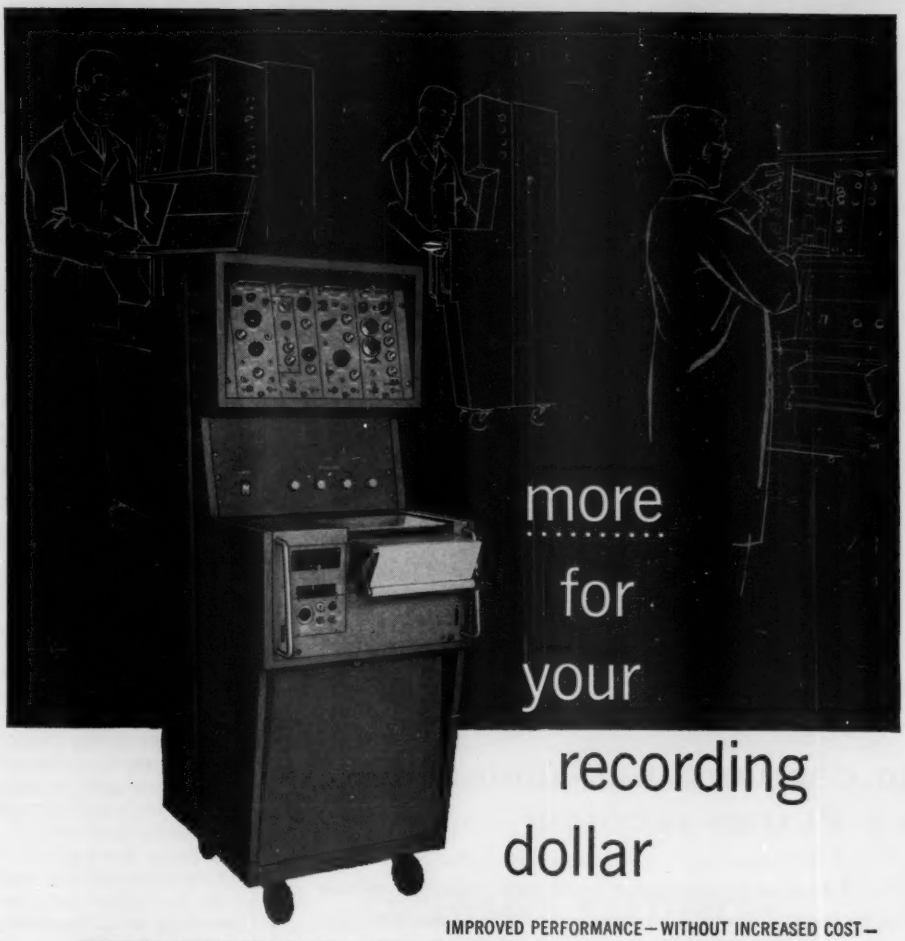
21 November 1960



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Letters

Expediency for Latin America

You ought to be congratulated for publishing the survey "U.S. assistance to Latin America" [*Science* **132**, 1936 (1960)]. I cannot help but offer some comments on what appear to me an array of contradictions.

Contradiction number 1. "The Act of Bogotá talks of very much the same . . . things: schools, literacy campaigns, low-cost housing, roads in the country districts. . . . None of these things, any more than any of Castro's original reforms, contributes much to real economic development." If the illiteracy in Latin America is to continue at the level of 50 to 90 percent, if the people are to continue to live in mud huts, sick and undernourished, and if whatever produce is extracted from worn-out soils cannot be taken to the market, how, I ask, can one reach the absurd conclusion that none of the things talked of in the Act of Bogotá contribute to real economic development?

Contradiction number 2. "A real program of development necessarily requires, for one thing, a drastic rise in tax rates, which in every Latin-American country are now far below those in any of the more developed countries." Taxation works wonders in countries with strong middle classes. Latin America, with the exception of Uruguay and Costa Rica and possibly Argentina, is characterized by the "have nots," with incomes of $100 to $500 per year, and the "have everythings," with incomes which approach the national budgets. I do not believe that the oligarchs will ever vote to tax themselves even at the existing rates, and even if they were to pay 1000 percent of what they pay now, the revenues would not be enough to solve any of the basic problems. What is needed first of all is a drastic rise in productivity and incentive to consume. To attain these basic goals, there must be technical know-how and literate, healthy workers. It is an irony to talk about democracy when the elementary ingredients of a decent living are denied the great majority of people.

I could go on analyzing the contradictions which appear to originate from ignorance of the real social and economic problems of Latin America. How can a government be progressive and popular and be able to institute sound economic programs if it does not (i) provide elementary education for the large masses of children who remain illiterate for the lack of schools, work for chronically unemployed people, a little sanitation and modest housing for the

have-nots, and roads to open new areas to productivity; (ii) find democratic formulas to institute land reform in countries where 100 families own 95 percent of the productive land; and (iii) obtain technical aid from the advanced countries and long-range, low-interest loans for broad economic development?

To formulate realistic policies of assistance, it is essential that the experts stop reading government reports, which are invariably colored by national pride, and go, instead, to see for themselves, not Buenos Aires, Caracas, or Quito, but the "people" in the "real countries." Policies based on expediency will bring only disaster.

L. A. ROMO

DuPont Company,
Edgemoor, Delaware

Racial Differences

The recent letter of Leon S. Minckler [*Science* **133**, 202 (20 Jan. 1961)] on racial differences points out the need for distinguishing between the scientific problem and the applications to daily life.

With regard to the scientific problem, Minckler admits that H. E. Garrett's opposition to the "equalitarian dogma" is probably justified, although the examples he gives have not been adequately studied. Concerning the problem of intelligence, with which Garrett is primarily concerned, nothing is said. Yet this is the crucial problem of the equalitarian dogma. For the results of mental tests almost invariably indicate significant differences in favor of whites, yet are disregarded by upholders of the dogma, who believe the differences to be due to differences in environment. What makes the problem especially difficult is (i) that we do not know how unfavorable an environment must be to stunt mental growth, and (ii) that, as Shuey has shown, the differences are greater at the upper socioeconomic levels, where the stunting effects should be the least.

As for the problem of application, I do not believe that Garrett or any other responsible psychologist would use racial differences as an "excuse for intolerance or discrimination." While Minckler does not give any example of his contention, it is probable that he refers to the question of school segregation, as that is central to the question of racial mental differences. But this problem is not as simple as it appears to be at first sight. For it is assumed that segregation is discrimination in favor of the whites, whereas the possibility that segregation could also help the Negro is entirely overlooked. Where

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differences do exist, to force Negroes to meet white standards is to do them no favor. Up north, both as student and teacher, I have been impressed by the Negro's difficulty in meeting white standards. I do not believe that the lower standards down south are the result of poorer teaching by Negroes. Teaching ranks relatively high as a profession among southern Negroes, and the teachers I have met appear to be eager to raise the position of the Negro by way of education.

RALPH W. ERICKSON
Department of Psychology, Mississippi State College for Women, Columbus

Literature Citation Counting

If one relies upon the enumeration of literature citations to identify significant research [J. H. Westbrook, *Science* **132**, 1229 (1960)], one should be reminded that this technique could be considered a special case of the more general opinion polls which so frequently are called into service to analyze economic and political aspects of our society. The pollster classifies and enumerates subjective opinions. Objectivity is introduced into the poll to the extent that opinion sampling is randomized. The more random the sampling is, the more objective will be the conclusion, and the more uncritical, by whatever criterion, will be the selection of opinions upon which the conclusion is to be based.

Thus it would appear that the enumeration of literature citations of a particular research article would represent the current value judgment of the pedestrian researcher in this particular area of research. Past experience has shown that favorable "average judgment" may lag decades behind the publication of brilliant but radical ideas.

Let us suppose that a scientist knew that he was to be rated according to the frequency with which his publications were cited. He might be extremely reluctant to enter any except the most popular areas of research. Such a system might be more vicious than that of rating him on the basis of the number of his publications.

Judging the merit of a scientific publication ultimately requires a subjective evaluation. Why place greater reliance on the "average judgment" than upon the judgment of those whose wisdom is most respected? To do so is to emphasize past accomplishment rather than potentialities for the future.

JAMES R. KUPPERS
Chemistry Department, Pfeiffer College, Misenheimer, North Carolina

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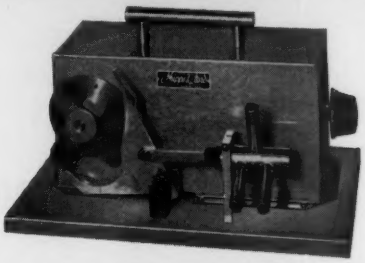

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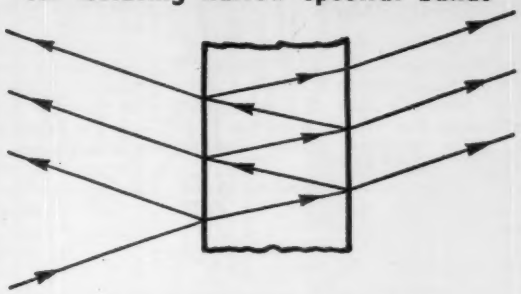

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Meetings

Forthcoming Events

May

5–7. American Soc. of Internal Medicine, Miami Beach, Fla. (G. T. Bates, 350 Post St., San Francisco 8, Calif.)

5–7. Wisconsin Acad. of Sciences, Arts, and Letters, 91st annual, Waukesha. (T. J. McLauglin, Secretary, 2865 N. Prospect Ave., Milwaukee, Wis.)

5–8. American Psychoanalytic Assoc., Chicago, Ill. (Mrs. H. Fischer, 1 E. 57 St., New York 22)

6–7. Academy of Psychoanalysis, annual, Chicago, Ill. (J. H. Merin, 49 E. 78 St., New York 21)

6–9. Circuit Theory, 5th midwestern symp., Urbana, Ill. (M. E. Van Valkenburg, Dept. of Electrical Engineering, Univ. of Illinois, Urbana)

7–10. American Inst. of Chemical Engineers, Cleveland, Ohio. (J. F. Van Antwerpen, AIChE, 25 W. 45 St., New York 36)

7–11. Institute of Food Technologists, New York, N.Y. (C. S. Lawrence, 176 W. Adams St., Chicago 3, Ill.)

7–12. Medical Library Assoc., Inc., Seattle, Wash. (Miss R. J. Mann, Mayo Clinic Library, Rochester, Minn.)

7–12. Society of American Bacteriologists, 62nd annual, Kansas City, Mo. (E. M. Foster, 311 Bacteriology, Univ. of Wisconsin, Madison 6)

7–12. Society of Motion Picture and Television Engineers, Toronto, Canada. (SMPTE, 55 W. 42 St., New York 36)

8–9. Titrimetric Methods of Analysis, symp., Cornwall, Ontario, Canada. [J. R. McCallum, Courtaulds (Canada) Ltd., Cornwall]

8–10. Aerospace Electronics Conf., 13th annual natl., Dayton, Ohio. (R. G. Stimmel, Institute of Radio Engineers, 1 E. 79 St., New York 21)

8–10. Instrument Soc. of America, Power Instrumentation Symp., 4th natl., Chicago, Ill. (H. A. Van Wassen, Duquesne Light Co., Pittsburgh 19, Pa.)

8–10. Mathematical Theories of Biological Phenomena, symp., New York, N.Y. (N. Rashevsky, Committee on Mathematical Biology, 5741 Drexel Ave., Chicago 37, Ill.)

8–12. American College of Physicians, 42nd annual, Miami Beach, Fla. (ACP, 4200 Pine St., Philadelphia 4, Pa.)

8–12. American Psychiatric Assoc., 117th annual, Chicago, Ill. (C. H. H. Branch, 156 Westminster Ave., Salt Lake City, Utah)

9–11. Western Joint Computer Conf., Los Angeles, Calif. (W. F. Bauer, 8433 Fallbrook Ave., Canoga Park, Calif.)

10–12. Production Engineering Conf., Toronto, Canada. (R. B. Larson, 5701 Carnegie Ave., Cleveland 3, Ohio)

10–13. National Science Fair—International, 12th, Kansas City, Mo. (Science Service, 1719 N Street, NW, Washington 6, D.C.)

11–13. Acoustical Soc. of America, Philadelphia, Pa. (W. Waterfall, 335 E. 45 St., New York 17)

11–13. American Inst. of Industrial Engineers, annual, Detroit, Mich. (W. J. Jaffe, Newark College of Engineering, 367 High St., Newark 2, N.J.)

11–13. American Radium Soc., Colorado Springs, Colo. (C. G. Stetson, 350 Engle St., Englewood, N.J.)

15–16. Co-ordination Compounds, symp., Hamilton, Ontario, Canada. (R. J. Gillespie, McMaster Univ., Hamilton)

15–17. Institute of Radio Engineers, natl. symp., Washington, D.C. (G. Shapiro, National Bureau of Standards, Washington 25)

15–17. Radiation Research Soc., annual, Washington, D.C. (E. L. Powers, Div. of Biological and Medical Research, Argonne National Laboratory, Argonne, Ill.)

15–18. Society of Aeronautical Weight Engineers, Akron, Ohio. (D. B. Block, 4004 Oxford Ave., NW, Masillon, Ohio)

15–18. Spectroscopy, 12th annual symp., Chicago, Ill. (W. Ashby, Continental Can Co., Inc., 7622 S. Racine Ave., Chicago 20, Ill.)

15–20. Conference on Nuclear Electronics, Belgrade, Yugoslavia. (J. Burt, International Atomic Energy Agency, United Nations, New York, N.Y.)

16–18. Western Conf. on Anesthesiology, biennial, Portland, Ore. (J. O. Branford, 2307 NW Overton St., Portland 9, Ore.)

(See issue of 17 March for comprehensive list)

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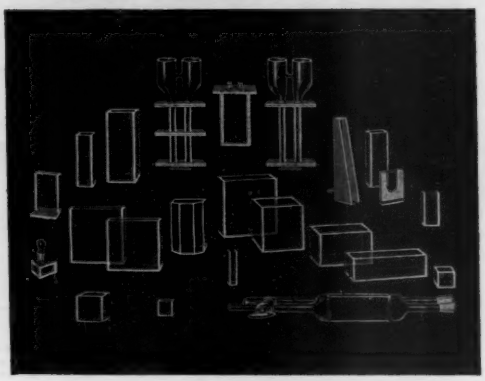

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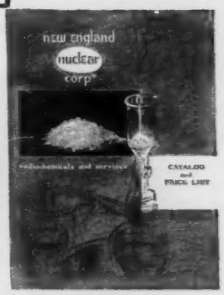

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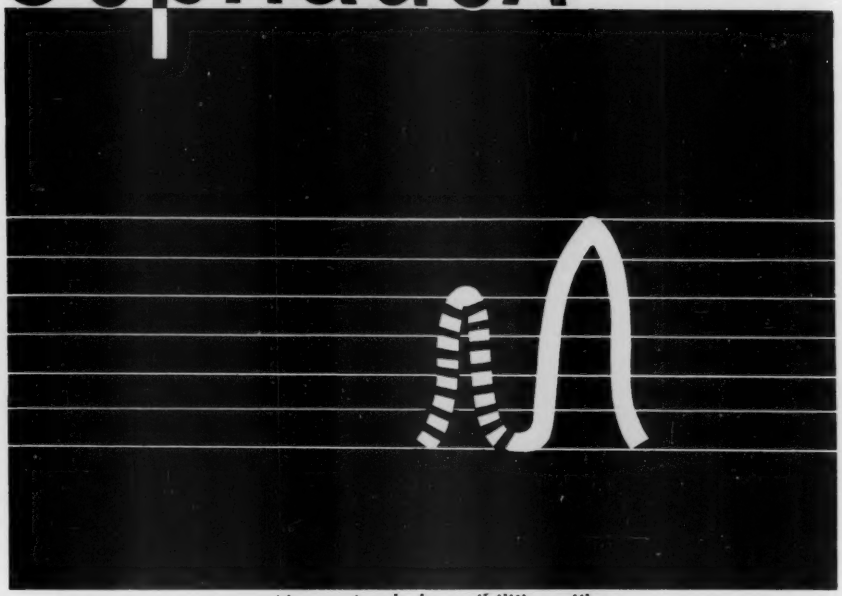

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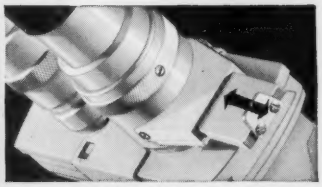
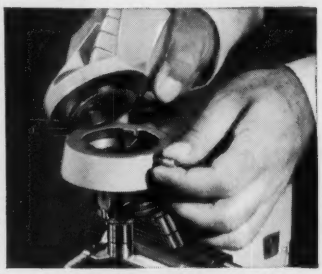
Here's why AO Phasestar offers you more!



New Bertrand Lens Built-in Bertrand lens is now supplied as standard on all binocular and trinocular Phasestars to provide new convenience. You slide lens into position in eyepiece tube and raise or lower to focus quickly on diffraction plate at back aperture-of objective.



Interchangeable, Rotatable Bodies Your choice of interchangeable, rotatable, monocular, binocular or trinocular bodies. Simply loosen thumb screw to rotate (full 360°) or interchange. Special inclined monocular body with vertical photographic tube also available.

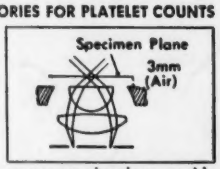
Interchangeable Stages You loosen single set screw to remove and interchange stages... your choice of graduated or ungraduated mechanical or MICRO-GLIDE stages.

The AO Spencer Phasestar is the most versatile phase microscope ever developed. Beginning with a basic Phasestar stand, you can select from a wide choice of interchangeable bodies, stages, optics and phase accessories to make up the specific model that meets your exact needs...all in all, more than 50 possible combinations or models to choose from. And whatever the model you choose, you're assured of superior AO Spencer quality *plus* matchless convenience, comfort and versatility.

Often particular phase applications require specific equipment and phase contrast objectives. Before you select your phase microscope and accessories consult your AO Sales Representative. He's qualified to help you select the microscope and equipment you need to do the job. There's no obligation, of course.

AO SPENCER PHASE ACCESSORIES FOR PLATELET COUNTS

The use of phase contrast for blood platelet counts is increasing in acceptance and application. The longer working distance required with the phase hemacytometer can be obtained with special AO Spencer long working distance accessories that provide up to 7mm working distance in air or 10½mm in glass.



SPENCER

American Optical Company

INSTRUMENT DIVISION, BUFFALO 15, NEW YORK

Dept. R-2

☐ I would like to review a specific phase microscopy problem. Please have AO Sales Representative call.

☐ Please send copy of Phasestar Brochure.

Name_____

Address_____

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